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THE

BRITISH AND FOREIGN

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PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

On the Structure and Use of the Spleen. By HENRY GRAY, F.R.S., Demonstrator of Anatomy, and Surgical Curator of the Pathological Museum, at St. George's Hospital.—*London,* 1854. 8vo, pp. 380. With Sixty-five Wood Engravings.

WHATEVER may be thought of the merits of the system of prize essays in the abstract, little hesitation can be felt by any member of the medical profession in affirming, that the institution of the Astley Cooper Prize has been highly advantageous to science, as well as honourable to the gentlemen to whom it has been successively awarded. To the admirable essays of Mr. Simon on the Thymus Gland, and of Mr. Wharton Jones on Inflammation, a third has now been added, which is certainly second to neither of its predecessors as to the evidence it gives of well-directed and laborious research, and which contributes many valuable materials towards the elucidation of the *quæstio vexata* that forms its subject. That Mr. Gray should have entirely succeeded in overcoming the difficulties of the investigation, and should have attained the full solution of a problem which has baffled so many able and zealous inquirers, it would be scarcely fair to expect; and we think it just to our author, as well as to our readers, to intimate at the outset, that whilst he has done much, he has also left much undone,—some of those very conclusions which he thinks he has most satisfactorily attained, being, to our minds, the most problematical.

The interest and importance of the subject, and the desire we feel, both on Mr. Gray's account, and for the sake of science, that the value of his labours should be justly appreciated, render it necessary that we should analyse his account of them with some minuteness. It is prefaced by a *Historical Introduction,* in which we naturally expected to find a complete summary of the anatomical results obtained, and the physiological doctrines propounded, by all the principal inquirers who had preceded the

author in the same line of investigation. But whilst very full in regard to the speculative opinions of the older writers, which can now be only referred-to as antiquarian curiosities, it gives but scanty information as to the labours of the most recent and pains-taking investigators. The memoir of Dr. Julian Evans, published in [illegible] on the Microscopic Anatomy of the Spleen, is mentioned with deserved commendations, and a summary is given of its contents; but of the numerous and important researches which have been subsequently made in the same direction, especially by Dr. Sanders and Professor Kölliker, scarcely any notice is here taken; some of their physiological conclusions being alone cited. It is true that these later researches are occasionally adverted-to by way of comparison, in the account of the author's own investigations; but it would have been far more convenient, as well as more satisfactory, had the historical summary been carried down to the date of his essay. And it would have also added greatly to the value of the book, had some notice been taken of the recent labours of Remak and Leydig, in preparing the essay for the press.

The *Development of the Spleen* is traced out with such completeness and detail, as to leave little for any one else to accomplish. The results of Mr. Gray's investigations on this subject had been partly communicated to the Royal Society, and published in the 'Philosophical Transactions' for 1852, in connexion with his parallel researches on the 'Development of the Ductless Glands' generally, of which we gave an account at the time;* but we find many important details here, which that paper does not contain; and there are, besides, several interesting illustrations. The first point established by Mr. Gray, is the original distinctness of the spleen from the pancreas, which he states to be very evident at the first appearance of the two organs, each arising from its own mass of blastema, although the increased size of both organs subsequently causes them to approximate so closely, as to have given rise in the minds of some of our best embryologists, to the idea that they formed but a single blended mass. Like other organs, the spleen is at first a homogeneous collection of nuclei and granular matter, which is developed in a fold of the intestinal laminæ; and which gradually undergoes increase and differentiation, whereby the several tissues and structures of the organ are evolved; part of these elements becoming developed into fibrous tissue to form the capsule and trabeculæ, part giving origin to bloodvessels and blood, whilst the greater part remains but little changed, to form the essential component of the organ, the "pulp-tissue." The Malpighian vesicles, which are such characteristic components of the spleen of the higher vertebrata, are not developed in the chick until near the completion of the period of incubation; being first seen as clusters of nuclei and fine granules, at the angles of division of the smaller bloodvessels, and upon the walls of the vessels themselves, to which they closely adhere; they are at first unenclosed in a special investing membrane, and do not acquire a proper envelope of any kind until some days after incubation has been completed. The following important physiological conclusions are drawn by Mr. Gray from the developmental history of the spleen:

* Vol. xii. p. 541.

"First, the small size of the spleen in the fœtus, as compared with its proportionate increase after birth, tends to show that it is not an organ the function of which is mainly exercised during *intra*uterine life. Second, the *entire absence* of any evidence, either of the *formation* of the blood-discs in the spleen (after its connexion with the general vascular system is effected), or of their *disintegration*, shows, I think, that it is neither a blood-forming nor a blood-destroying gland, at least during *fœtal* life. Third, in the pulp-parenchyma, a distinct process of cell-growth, of ripening, and of cell-destruction has been observed, and these processes have been seen to occur concomitant with the evolution of the vessels of the gland; that of cell-growth occurring with extreme rapidity as soon as the arteries which supply the organ are formed; then that of ripening, and of cell-destruction, taking place to the greatest extent up to, and during the time that the development of the splenic veins takes place. This would seem to show that some secretion took place in the gland, which became collected in it, ready to be removed by the veins as soon as their development should occur. Such a process is always to be found going on in man and animals, though ever varying in extent at all periods." (pp. 70, 71.)

Mr. Gray further mentions the important fact, that a distinct yellowish-green bile is found in the gall-bladder of the fœtal chick, at a period *considerably antecedent* to the development of the splenic vein, and its connexion with the vena portæ; so that the colouring matter of the bile cannot be formed, as supposed by Kolliker, entirely at the expense of blood-corpuscles which have undergone disintegration in the spleen.

The weight of the human spleen, towards the end of fœtal life, as compared to that of the entire body, averages about 1 : 350, whilst in adult life it varies from 1 · 320 to 1 : 340, or 1 : 400. In old age, on the contrary, the organ not only decreases in *absolute* weight, diminishing from an average of seven ounces, to an average of four ounces and a half; but it also diminishes in *proportional* weight, its average being only about 1 : 700. From the comparison of 160 observations on the weight of the spleen at different ages, therefore, Mr. Gray comes to the following conclusion:—"That the spleen attains its largest size and exerts its peculiar function [most energetically] between the periods of birth and the later periods of adult life; or, in other words, *during the most active periods of growth and nutrition of the body*." But as the same comparison serves to show, that the range of variation, both above and below the average, at each period of life, is a very wide one, and is far greater than can be fairly ascribed to individual peculiarity, the question arises, Whether this variation can be legitimately ascribed to differences in the condition of the organ, either as to the diverticular function ascribed to it by Stukely, Dobson, and others, or as to the activity of its glandular operations, and the supply of blood which it may receive for this purpose. As this is a question which needed to be experimentally investigated, Mr. Gray had recourse to the rabbit; and from a comparison of the weights of the spleens of thirty individuals of this species, at different periods after feeding, he has deduced what appears to be the fair conclusion, "that the weight of the spleen *increases considerably* during the time when the digestive process is near to its completion, at the time when the new material is about to be, or has become, converted into blood; and that it *decreases considerably* in weight at varying periods *after* that process has been finally completed." This conclusion is confirmed by the results of experiments upon highly-fed and upon starved (or rather, we presume, insufficiently fed), animals

of the same species; for the increase during digestion was carried in the former to an unusual degree, so as to give the organ more than twice its normal weight, whilst in the latter, no increase after feeding was observable.

Having thus studied the organ as a whole, and shown what conclusions may be drawn from its comparative dimensions under different circumstances, Mr. Gray proceeds to a minute examination of its *Anatomical Structure.* Through this we shall not follow him in any detail, since a large part of his descriptions merely repeat what was previously well known; but we shall select those points on which he has added to our previous information, or as to which he is at issue with preceding observers. His account of the fibrous tissues of which the fibrous capsule and trabeculæ are composed, corresponds in the main with that of Professor Kölliker and other histologists; and Mr. Gray agrees with previous experimenters, moreover, in regard to the very low degree of contractility which these tissues exhibit. He has submitted the spleens of sheep, oxen, dogs, and cats to a strong magnetic current, within a short time after death, when the same current produced the most marked contractions in the œsophagus, the large and small intestines, and the bladder; and no effect whatever was produced on the spleens of sheep and oxen; whilst in those of dogs and cats, a slight wrinkling of the surface was produced, though the diameter of the organ was not sensibly lessened, and no blood was expelled from it. No such decided contractions were witnessed, as have been asserted by some observers to take place; and it is strongly to be suspected that these statements were made under the influence of a preconceived idea of the muscularity of the organ, and of its agency in the propulsion of the blood. It would have been more satisfactory, however, if Mr. Gray had used the interrupted current of the magneto-electric apparatus, as well as the continuous current of the ordinary galvanic battery, which is what (from his designation of it) we presume him to have employed in these experiments.

Mr. Gray next proceeds to describe the *Bloodvessels* of the spleen, his account of which corresponds in most particulars with that of his predecessors, but contains some important points of novelty. The walls of the capillaries, he says, lie in immediate contact with the pulp-tissue, by which means this substance is constantly exposed to the influence of the fluid ingredients of the blood. Generally speaking, the capillaries may be traced into direct continuity with the smaller veins, which suddenly increase greatly in size after the junction of the capillaries with them, and have walls of even greater delicacy than those of the capillaries themselves. Some of the capillaries, however, cannot be traced into direct continuity with veins (as Ecker had previously ascertained), but gradually becoming reduced in size, their wall becomes more delicate, and is finally lost; the injected material then escapes into interspaces in the pulp, the walls of which are formed merely by its components; and they appear finally to communicate with the veins by intercellular spaces,—thus presenting us with an example (which, so far as we yet know, is a solitary one) of the exceptional existence of that *lacunar* circulation in man, which is the normal type in the lower invertebrate animals. As this is a point of first-rate importance, and Mr. Gray's conclusions in regard to it

are not in accordance with those of most of his predecessors, we should have liked more precise information as to the mode in which he examined these capillaries, and satisfied himself of their frequent want of distinct walls. The veins originate in three modes:—(1) by direct continuity with the arterial capillaries; (2) by lacunar spaces; (3) by cæcal pouches. Of these modes of origin, the second is stated by Mr. Gray to be by far more frequent than either of the other two; and its physiological relations are, as he justly observes, of very great importance, since they afford a ready explanation of the *constant occurrence* of diffusion of blood, and of its disintegration in the pulp-tissue of the organ, which he believes himself to have substantiated. The origin of the veins by cæcal pouches is not frequently to be observed; but the existence of these pouches (which usually consist of vessels of about $\frac{1}{250}$th of an inch in diameter, but of variable length, generally situated on the side of a vein just before its smallest branches are given off) is of interest in relation to the supposed diverticular function of the venous system of the spleen. The veins, in their course towards their main trunk, increase in diameter beyond all proportion to the size of the branches they receive; thus, two branches, each of $\frac{1}{1250}$th of an inch in diameter, formed a trunk of $\frac{1}{500}$th of an inch; and a little further, this trunk had increased to the diameter of $\frac{1}{250}$th of an inch, without receiving any accessory branches. Thus it appears that, even in the human spleen, there is a provision for the reception of very varying amounts of blood into the venous system; the small as well as the large branches of which are supported by a highly elastic and distensile framework of fibrous tissue.

This provision, however, is far more obvious in the spleens of Ruminant and Pachydermatous mammals; for the fibrous sheaths which envelope the smaller veins in man, even to their minute subdivisions, and which form a part of the general trabecular system, are here wanting on all but the principal branches of the main trunk; and even the proper coats of these vessels thin away, until scarcely anything is left, even in veins of considerable dimensions, but the smooth lining membrane covered with a layer of fusiform epithelial cells. The finer microscopic veins consist of nothing else than this membrane with its epithelium; and these end in the before-mentioned lacunæ, in which the membrane and the epithelial cells can no longer be traced, so that nothing separates the blood from the cells of the pulp between which it meanders.

Mr. Gray's next section treats of the *Blood* of the spleen; but as it seems to us much more appropriate to proceed first to the proper anatomical components of the organ itself (since the question of the alterations which it effects in the blood passing through it, would bring us at once to the consideration of its function, which can be only fittingly discussed after its structure has been fully inquired into), we shall pass this by for the present, and go on to the description of the *Spleen-Pulp*, the *colourless* and *coloured* elements of which have to be separately considered. Mr. Gray's account of the former corresponds in the main with that given by Kölliker, Sanders, and others; and he is led by a comparison of the various forms and conditions presented by the components of the splenic parenchyma, to the conclusion at which they had previously arrived—

"Namely, that a continuous process of cell-development, of cell-growth, and

decay, takes place in the pulp of the spleen, during which, nuclei are formed, around which a blastema, or, in other cases, a cell-growth is produced, after which the nuclei disappear, and the cell-membrane and its contents become broken up and vanish." (p. 182.)

He has made an important step in advance, however, by experimentally ascertaining that "the parenchyma-cells of the organ taken together, exist in *by far larger quantity,* and form a very *considerable part* of the entire bulk of the spleen, in all animals in which the nutrition of their bodies is in a most perfect condition, and more particularly in those in which the addition of new material *exceeds* that required by the waste of the body;" whilst "the parenchyma-cells *not only diminish,* but actually *do not exist at all,* in those animals in which new material has not been supplied in such quantity as was required for the waste of the body."

The resemblance between the parenchyma-cells of the spleen, and the *colourless* corpuscles abundantly contained in the venous blood returning from the organ, is so close, that it is difficult not to regard them as identical; and the passage of the blood through lacunæ in this parenchyma, without any membranous limitation, seems fully to account for the presence of some of its cells in the return current.—The *coloured* components of the spleen-pulp are affirmed by Mr. Gray to be nothing else than *red corpuscles* variously modified.

Blood-discs have been recognised in the midst of the pulp cells, by every one who has microscopically examined the parenchyma; but they have been usually considered as no more forming part of the proper components of the spleen, than they would be held to do in the case of nerve or muscle. Mr. Gray, however, being led by his view of the nature of the splenic circulation, to the conclusion that there is no distinct separation between the current of blood and the substance through which it passes, regards even normal blood-discs as proper constituents of the spleen-pulp, just as he regards the colourless cells of the parenchyma as normal constituents of the venous blood of the spleen. The great majority of the coloured corpuscles, however, are *altered* blood-discs; and the following is his account of the principal changes which they undergo:

"First, some blood-discs may be observed (and these are somewhat numerous at certain periods), whose size is generally smaller (they vary from the 5500th to the 7000th or 10,000th of an inch), their form variable, circular, or more generally of an irregular circular or oblong form, or indented and wrinkled. Their colour is of a deep scarlet or orange-red, and their outline very dark and highly refractive. These changes may be observed either in single blood-discs, or almost as commonly in small or large heaps, or masses of them collected together; and when such occur in masses, they are of a deep red colour, or reddish-brown, quite distinct in colour from heaps of unchanged blood-discs. In other cases, these single discs may be observed to become still more irregular and indented in form, of a darker red colour, and finally subdivide or break up into small, generally circular, minute, dark red, reddish-brown, or black granules; whilst the heap of globules undergo a similar transition into granules, which either exist separately, dispersed in the substance of the pulp, or as masses of such dark red granules. Such, there is no doubt, is the most frequent method by which the blood-corpuscles in the spleen are disintegrated.

"Secondly. There are observed, although rarely, perfectly unchanged blood-discs contained in a cell; these discs present their usual *normal* appearances. . . . In none of these could I ever detect a nucleus on the wall of the enclosing cell,

which is exceedingly fine and delicate, transparent, and homogeneous in texture. The number of the contained discs varies from one to three. I have never observed more of this variety.

"The more frequent form, however, in which the cells containing blood-discs present themselves is the following: They are large vesicles, which vary considerably in size; their average diameter is $\frac{1}{2500}$th of an inch, the largest $\frac{1}{1500}$th, the larger size being the more frequent. Their form is generally spherical, sometimes oval or oblong. They consist of an external membrane, perfectly transparent, which *sometimes* contains on its wall a distinct nucleus of a circular form, with a central nucleolus, the nucleus being about the size of an ordinary blood-disc. In many no nucleus can be seen. In the cavity of the vesicle may be seen from one to nine or ten somewhat altered blood-discs, that is to say, blood-discs of a circular or somewhat irregular circular form, of a *deep orange-red* colour, with an outer dark refractive margin, which sometimes presents a wrinkled edge; they do not present also the same distinctly flattened appearance that normal blood-discs do. Others are observed, in which the contents consist partly of the above-described altered blood-discs, one or more of them, however, presenting indented edges, as if previously to their breaking up into dark-reddish granules. Lastly, other vesicles may be seen, containing a varying number of small granules, from four or five to twenty or thirty in number; these granules are of an irregular form, with dark refractive margins, and of a pale red, or dark red, or reddish-yellow colour, or the vesicle contains a mass of colourless granules. These structures, in one or the other form, constitute one of the main elements of the pulp.

"But besides these, there are interspersed throughout the whole of its substance granules of varying form and size, in large quantities; and these are precisely similar to those found in the previously-described cells, and which consequently appear as the débris of the disintegrated blood-discs. They vary in size; some are exceedingly minute fine granules, others exist as single corpuscles about the size of the blood-discs, or aggregations of such, or in various sized masses; their colour is a deep red, or reddish-yellow, or black, and their margins are dark and refractive. In some cases these corpuscles are so numerous as to form the chief constituent of the pulp, at other times, again, they exist only in sparing numbers. Occasionally a large number of *reddish crystalline* forms are found in the substance of the pulp, in the place of the above-described corpuscles. They exist either as free crystals, of an acicular form, exceedingly minute in size, which, when seen separately, have a pale red colour, or of masses of such crystals aggregated together, when they present a brilliant red colour." (pp. 186—189.)

All these changes obviously tend towards *disintegration;* and thus, in Mr. Gray's belief, the coloured constituents of the spleen-pulp wholly consist of red blood-discs in various stages of that process. After numerous observations, however, he has only observed, in man, two cases of blood-corpuscles included in cells, and then only in small numbers; so that the degenerating process in the human spleen takes place without any such agency. In the sheep and pig, he has never observed any disintegration of the blood-discs; in the rat, the disintegration shows itself solely in free corpuscles; in the rabbit, on the other hand, it takes place partly in those included within cells; in the horse and ass, both forms of the process may be observed more perfectly and completely than in any other mammalia. These variations are very instructive, both as accounting in some degree for the very discrepant statements which have been put forth on this subject, and also in their physiological bearing.—We draw from a later section on the Comparative Anatomy of the Spleen, the following additions as regards the manner in which the process is effected in Birds, Reptiles, and Fishes.

The spleen of Birds, though of smaller proportional size than that of mammals, is made up of the same elements; and the fact that the coloured portion of the pulp consists of blood-corpuscles in various states of disintegration, is most distinctly traceable. In some instances, the chief alteration is in size and form, the margin and surface of the corpuscle being more or less corrugated, so that its diameter is diminished, whilst its colour is darker than usual; in other cases, the nucleus and cell-wall of the corpuscle remain unchanged, but the colour has faded. These degenerating corpuscles are often collected into heaps, of varying form and size; and these are sometimes enclosed by a membranous investment. In a few cases, Mr. Gray has seen in the spleen of birds a number of small, elongate, rod-like, crystalline bodies, of a pale reddish colour; these either presented a perfectly straight form, or were somewhat curved; they either existed free, or were contained in unchanged or partly-changed blood-corpuscles. In Chelonian and Ophidian Reptiles, both arteries and veins form a distinctly *closed* plexus, both on the surface and also through the interior of the organ; and no trace of disintegration of blood-corpuscles, either singly, or in cells, has been observed by Mr. Gray, in repeated examination of their spleen-pulp, although he states that small, dark-coloured, granular masses, sometimes enclosed in a vesicular envelope, form a considerable part of the parenchyma. In Batrachia, on the other hand, the plexiform arrangement of the bloodvessels does not exist, and the smaller venous channels are mere interspaces in the substance of the pulp; and in these animals, the process of disintegration of blood-corpuscles may be distinctly made out, the large size of the corpuscles rendering the observations more easy and more certain.

"Some of the blood-discs present their usual form, size, and colour, containing in their interior a small circular-shaped granular nucleus. Others are observed of *smaller size*, of *darker colour*, and having a *circular* instead of an oval form. The margins of these present a wrinkled corrugated appearance, the nucleus either remaining at first unchanged, or else losing its granular form, and becoming more pellucid. This wrinkling or crumpling up of the blood-corpuscle is not confined to the margin, but extends throughout its substance, altering the normal form of the blood-disc to such an extent, as not readily to be recognised until the above-mentioned stages have been observed. This wrinkling and corrugation proceeds until the corpuscle becomes an irregular dark red or reddish-brown jagged mass, the substance of which has become changed into a coloured pigment granule. In the interior of some of the blood-discs which are devoid of nuclei, may be seen three or four minute brilliant reddish granules of hæmatin, which sometimes assumes a rod-like form. Similar reddish-brown or black granules, but of larger size, which exist either singly or collected into large masses, are also to be seen disseminated through the substance of the pulp. In *no single instance*, in at least fifty examinations that I have made, examining the organ in every possible variety as regards nutrition, *have I ever been able to observe the existence of the disintegration of the blood-corpuscles in cells*, as described by Kölliker." (pp. 319, 320.)

Now there is not here the discrepancy, which at first sight appears to exist. For Professor Kölliker does *not* speak of the blood-corpuscle-holding cells as components of the *spleen-pulp* of batrachia; but distinctly states that they are contained in the bloodvessels, and that he has traced them into the vena portæ and its hepatic branches, and even into the vena cava. He adds: "In any case, these facts may be considered as

conclusive of the not unfrequent occurrence and formation of the cells in question within the bloodvessels of the spleen; although it can scarcely be added that they are not probably also formed in the extravasated blood."* Mr. Gray, on the other hand, does not seem disposed to admit that the disintegrating changes, and the formation of cells around blood corpuscles, ever take place within the bloodvessels of the spleen; but regards the disintegrated corpuscles and blood-corpuscle-holding cells, which he may find in the blood of the splenic vein, as proper components of the spleen-pulp, which have found their way into the vessels, through the want of a limitary membrane. (p. 195.) The difference, therefore, does not seem to be so much one of facts, as of the interpretation of them; and if the blood-corpuscle-holding cells are to be frequently found, as Professor Kölliker states, in the splenic vein and vena portæ of batrachia, whilst they are *not* to be found, as Mr. Gray affirms, in the spleen-pulp of those animals, a strong case seems to us to be made out for their formation within the vessels, as affirmed by Professor Kölliker. Mr. Gray seems to have overlooked the significant fact stated by his predecessor, that in Triton igneus, whose spleen is tolerably transparent at its margin, the blood-corpuscle-holding cells are frequently to be seen occupying the capillaries in rows, and may be driven into the larger venous channels by pressure. In the spleen-pulp of Fishes, Mr. Gray has constantly met with blood-corpuscles in progress of degeneration, presenting the same double set of appearances as those already described; he has never, however, seen these blood-corpuscles enclosed in cells, although he has occasionally found dark granular masses, such as are formed by the disintegration of the corpuscles, enclosed in a distinct vesicular envelope. In the tench, bream, barb, and eel, he has observed the formation of rod-shaped crystals within the corpuscles; and as similar crystals are found disseminated, sometimes singly, sometimes in masses, throughout the entire substance of the pulp, it is pretty obvious that they have been originally formed within the blood-corpuscles, and are set free by the disintegration of their enveloping membrane.

It may seem to have been substantiated, then, by Mr. Gray's observations, that blood-corpuscles, either singly or in masses, become disintegrated when effused into the spleen-pulp, without the necessity of their inclusion in cellular envelopes; that blood-corpuscles do become enclosed in simple vesicles, which form in the plasma around them, without the existence of a nucleus; and that blood-corpuscles also become enclosed in distinct nucleated vesicles, the formation of a nucleus of the cell preceding, in this case, that of the investing membrane. We must remark, however, upon the whole description, that we are entirely unable to determine, from Mr. Gray's account of his observations, how he distinguishes the corpuscles of the spleen-pulp from those contained within its vessels. Every one who has himself made a microscopic analysis of the spleen, must be aware of the difficulty of discrimination on this point; and we should like to know by what criterion Mr. Gray has made his determinations. Not only Mr. Wharton Jones, but Remak and other excellent observers, hold the pigment-cells of the spleen to be *sui generis*, and

* Cyclopædia of Anatomy and Physiology, vol. iv. p. 784.

altogether deny that they can be regarded as degenerated blood-corpuscles. And all the blood-corpuscles which may be recognised as such, they refer to the capillary passages, not to the pulp of the spleen. Mr. Gray tells us that he has endeavoured to determine by an extensive series of observations, the circumstances under which these changes take place; and he *found, that whilst the proportion of blood-corpuscles undergoing disintegration in the spleen-pulp did not seem to have any relation to the period* of the digestive process, it was obviously affected by the state of the animals as regards their nutrition; the number of coloured elements (and especially of those blood-discs in which, as they present their normal characters, the degenerating process may be presumed not to have yet commenced,) being much greater in well-fed than in ill-fed animals. We shall hereafter see that further information upon this point may be derived, from the comparison of the blood of the splenic vein with that of the splenic artery. With reference to the question whether like disintegrating changes take place in the blood-corpuscles of other organs, Mr. Gray states that he has never obtained any evidence of them either in man or in other mammalia, in birds, in reptiles, or in most fishes; but that in the bream, carp, and tench, he has frequently noticed the same appearances in the kidneys and liver, as are to be met with in their spleen. It would be a matter of peculiar interest to determine whether the circulation in these organs has anything of that *lacunar* character in the animals in question, which it seems to possess in the spleen of nearly all vertebrata.

The *chemical composition* of the spleen-pulp has been carefully investigated by Mr. Gray, with the assistance of his colleague, Dr. H. M. Noad. We have only space for the most important results of their inquiries, which seem to have been prosecuted on a very extensive scale.—The chief component of the spleen is an *albuminous* product, of which the colourless portion of the pulp is almost entirely made up; as is shown by the effects of reagents upon them under the microscope. A large quantity of colouring matter, identical in all its properties with the hæmatin of the blood, is also present; and from what has been already stated, it scarcely appears doubtful that this is furnished by the disintegrating corpuscles which constitute the coloured portion of the pulp. The rod-like, crystalline bodies seem to consist of the hæmatoidin of Virchow. No relation was traceable, by the effects of reagents, between the colouring matter of the spleen and that of the bile. As much as sixteen per cent. of iron was found in the ash, and also no less than forty per cent. of phosphoric acid; and the large proportion of the latter, as well as of the former, of these substances is significant of their derivation from blood-corpuscles, which contain nearly all the phosphorized fats of the blood. The presence of *lactic acid* in the spleen-pulp, as asserted by Scherer, has been verified by Mr. Gray and his coadjutor: but, notwithstanding that they have followed Scherer's process (implicitly, as they believe), and have operated upon so large a scale that in one of their experiments the spleens of twenty-five oxen were submitted to analysis at once, they have wholly failed in detecting either *uric acid*, or its related compound, *hypoxanthine*, the latter of which substances Scherer states that he found in the human spleen at all ages, as well as in the substance

of the heart. The reputation of Prof. Scherer as an analytic chemist is such, that the negative results obtained by Mr. Gray and Dr. Noad do not allow us to entertain a doubt of the validity of his positive conclusions; and we can only suppose that the discrepancy may have arisen, either from some apparently unimportant difference in the analytical process employed, or from some diversity in the mode of nutrition or general condition of the animals experimented on.

The *Malpighian Corpuscles* of the spleen still remain to be considered. As regards their general position and relations, Mr. Gray has little that is new to tell us. Like Kölliker, he considers these bodies to possess an essentially distinct membranous investment, formed by the interlacement of fibres (chiefly of the white or non-elastic kind) derived from the sheaths of the arteries to which they are attached, the irregular meshes left between these being filled up by an exceedingly delicate and finely granular membrane; and he considers the cellular structure which has been described by Dr. Sanders as existing in it, as a mere optical illusion. He does not tell us upon what animals his observations have been chiefly made, but simply remarks, that in the human subject the pale white fibres are somewhat less distinct and are smaller than in other mammalia. This is a point of considerable importance, as we shall see further on, when the signification of these corpuscles comes to be considered. With regard to the vascular supply of the corpuscles, Mr. Gray does not make any decided advance upon the description given by Müller in 1834; for while he describes some of the arterial branches as occasionally passing through the centre of these bodies, he makes no mention of any other plexus of capillaries than that covering their external surface, which he considers quite sufficient to bring the blood into relation with their contents. Now upon this important point, Kölliker tells us that from a single observation in the spleen of a cat, he is inclined to regard the pulp of the Malpighian corpuscles to be traversed, like that of the Peyerian follicles, by fine (capillary?) bloodvessels.* Dr. Sanders, in a paper communicated to the Physiological Society of Edinburgh, in January, 1852, and published in the 'Edinburgh Monthly Journal,' for March, 1852, stated that by boiling the tissue in acidulated water, drying it, and then cutting thin sections, he had not only determined the passage of arterial twigs diametrically through the substance of the follicles, but had also perceived in their interior "stains of blood, often in linear arrangement, indicating capillaries." Of neither of these statements does Mr. Gray take any notice whatever; and the question does not seem to have at all attracted his attention. Much light has been recently thrown upon it, however, by Mr. Huxley, who, in a paper 'On the Ultimate Structure and Relations of the Malpighian Bodies of the Spleen and of the Tonsillar Follicles,' in the 'Quarterly Microscopical Journal,' for January, 1854, and in the notes to the English edition of Prof. Kolliker's 'Handbook,' tells us that the existence of a capillary network in the interior of the Malpighian follicles may be made out with the utmost ease by a proper method of examination; all that is necessary being to make, with a sharp knife, a tolerably fine section, containing a Malpighian follicle: to spread it out with needles, adding nothing but a little weak syrup; and then,

* Handbuch der Gewebelehre des Menschen, p. 438.

after placing a glass plate over the section, to apply a gentle and gradual pressure, just sufficient to render the bodies transparent. It is easy, he tells us, by sliding the plate with a needle, to cause the bodies to roll a little on their axes, and thus to convince oneself, by the relative position which the bodies and the vessels assume, that the latter do really pass through, and not merely over, the latter. We have ourselves verified Mr. Huxley's observations on this point; and having had no difficulty in arriving at the same conclusions, we are the more surprised that Mr. Gray should have passed the matter by.

Like his predecessors, Mr. Gray describes the contents of the Malpighian corpuscles as consisting of granular blastema, nuclei (some of them of an elongated oval form, and containing a variable number of pale granules), and nucleated cells, in all essential particulars corresponding with the colourless components of the splenic pulp. The diversities observable in the relative abundance of these components in different individuals and under different circumstances, are held by Mr. Gray, and we think quite justly, to indicate that "a continuous process of cell-development, of cell-growth, and dissolution, takes place in the splenic corpuscles, as in the pulp; wherein nuclei are formed, around which a granular plasma is arranged, and after which, both nuclei and plasma break up and disappear, probably forming the amorphous blastema in which the nuclear structures are contained."

A very valuable series of researches has been made by Mr. Gray, chiefly on cats, rabbits, and rats, with regard to the conditions which affect the development of these corpuscles, and the amount of their contents. The first of these he found to be the *state of nutrition of the animal generally;* the maximum of size being attained in all instances in which the animals had been highly fed, so that a greater amount of new material was added to the system than was required for the expenditure and waste of the body; whilst in ill-fed, and more particularly in starved animals, they were invariably reduced to their minimum of size, and were sometimes almost totally absent. Hence, as the contents of these corpuscles, like the spleen-pulp generally, consist of an albuminous compound, it may be reasonably surmised that they store up a portion of the surplus alimentary material, when (to use Mr. Gray's rather slovenly phraseology) "the system is in a highly nutritious state," restoring it again to the blood during the demand set up under the opposite condition. But further, Mr. Gray found that the *period of the digestive process* greatly modified the state of repletion of these bodies; their contents being most abundant during the latter part of the act and for a short time after its completion, while they are most scanty long after its completion and during its early stages; little increase is produced in them, however, by the accomplishment of the digestive act, in animals which have been for some time underfed. The *nature of the aliment,* again, affects the development of these corpuscles; for when rats which had been previously kept upon a mixed diet, were fed for several days upon boiled white of egg (on which they throve well), the whole spleen acquired a large size, its weight being to that of the body as 1:250, and the Malpighian corpuscles were peculiarly obvious; whilst in other rats, fed upon lean meat, upon fat, or upon gelatin, on neither of which diets did they

live long, the spleen was found of less than half the size, and its Malpighian corpuscles were so small as not to be visible to the eye. The *supply of water*, moreover, was found to modify the size of the corpuscles, and the fluidity of their contents.

Mr. Gray argues that, notwithstanding the absence of a distinct limitary membrane, and other structural differences, the Malpighian corpuscles should be regarded as analogous to the follicles of ordinary glands; but he seems to overlook the exact correspondence of their contents with the constituents of the pulp in which these corpuscles are imbedded; and he does not attempt to account for the isolation of these contents from the surrounding mass, nor to show what functional relation they bear to it. This question has recently been considered by Remak* and Leydig;† and their views have been adopted by Mr. Huxley, who has confirmed them by his own researches, with which, too, the statements of Mr. Wharton Jones are in accordance. All these observers agree in denying to the Malpighian bodies any proper membranous capsule; but affirm that their boundary consists only of a part of their own tissue, more or less metamorphosed into fibres, together with, in some instances, a layer of proper white and yellow fibres derived from the coats of the arteries to which they are attached; so that the wall of the Malpighian corpuscle is not separated by any distinct line of demarcation, either from its contents or from the surrounding red pulp. As such is even more evidently the case in the lower vertebrata than it is in the mammalia, we cannot but regret that this part of the comparative anatomy of the spleen has been so incompletely worked out by our author. He correctly tells us, indeed, that the Malpighian corpuscles of birds, which are very abundant, are destitute of any fibrous investment derived from the sheaths of the arteries, and are bounded only by "a fine and exquisitely delicate membrane, which is transparent, homogeneous in texture, presenting in some cases an exceedingly fine dark granular texture," as if made up of condensed granular matter resembling that which it encloses; but he states that, although Müller detected these bodies in the Chelonia, and Oesterlen in the naked Amphibia, he has not been able to discover their existence in any animals belonging to the class of reptiles. Now it seems very probable, from the researches of Leydig and Remak, that in those reptiles whose splenic sanguiferous plexus is entirely closed, and whose spleen-pulp contains no disintegrating blood corpuscles, there are no Malpighian corpuscles; the signification of this fact being, that the whole spleen is, so to speak, a vast Malpighian body, its composition being such throughout, as is found, in the spleens of higher animals, only in the encysted portions. But it is not true that no representative of the Malpighian corpuscles exists in the spleen of Batrachia; for although not distinctly marked out by a special investment, a collection of colourless pulp lies in the midst of the red pulp which forms the cortical portion of the organ, and into which it passes continuously. Among Fishes, various remarkable modifications present themselves in the condition of these organs. The Malpighian bodies do not always hold their usual relation to the arteries; but they sometimes appear to be quite free in the midst of the spleen-

* Ueber runde Blut-gerinnsel und über Pigment-kugel-hältige Zellen: Müller's Archiv, 1852.
† Anatomische Untersuchungen über Fische und Reptilien.

pulp; whilst in other instances (as to which Mr. Gray is in accordance with Leydig), their components are dispersed, without any special investing capsule, through the walls of the vessels; and in the cod (according to Mr. Gray), they are scattered through the whole substance of the pulp. It is curious that in fishes generally, the contents of the Malpighian corpuscles, or what corresponds to them elsewhere, are deeply coloured; nevertheless Mr. Gray assures us that, after the most repeated and careful examination in numerous fishes, he has never been able to detect among them either normal or changing blood-discs; and he regards the colouring matter, not as hæmatin, but as a true secretion, formed in distinct nucleated cells, and in some respects corresponding with that of the bile. It is remarkable that similar agglomerations of nucleated vesicles containing coloured particles are found in many fishes on the walls of the bloodvessels of the liver.

Taking into account, therefore, the diversified forms under which the Malpighian corpuscles or their representatives present themselves,—and connecting with this the evidence afforded by the experimental observations of Mr. Gray, as to the functional correspondence between the contents of the Malpighian corpuscles of mammalia, and the colourless portion of their spleen-pulp (as shown in the marked increase of both at the end of the digestive process, and the diminution of both by deficient nutrition), —we give our full assent to the view of their nature advocated by Remak, Leydig, and Huxley; namely, that they are not to be likened to the acini or follicles of ordinary glands, but that they simply consist of portions of the splenic pulp more or less isolated from the rest. We must, as Remak urges, consider the spleen to be formed of two principal constituents; the first being its parenchyma; whilst the second is a superadded fabric of bloodvessels, nerves, lymphatics, elastic and contractile elements. The manner, however, in which the latter are arranged in and about the parenchyma, is, in a manner, accidental; and varies considerably in different animals. The "indifferent tissue" (or granular blastema containing nuclei and cells in various stages of development) may be *intercapillary*, as in the pulp; it may be specially limited to the *medullary* portion of the organ, as in certain amphibia; it may be *vaginal*, that is, dispersed through the coats of the arteries, as in some fishes; or it may be *encysted*, as in the most characteristic forms of Malpighian corpuscles, which are still to be regarded, however, as offsets from the walls of the bloodvessels. The following extract from Mr. Huxley's paper will show the importance of this view in its relation to general histology, which has been recently dwelt on by several writers, but has been especially enunciated and explained by Leydig.

"There is no line of demarcation to be drawn between the spleen, the lymphatic glands, Peyer's patches, the glandulæ solitariæ, the supra-renal capsules, the thymus, and the pituitary body; but these form one great class of glands, characterized essentially by being masses of 'indifferent tissue' contained in vascular plexuses, which, therefore, may well retain their old name of vascular glands. The primary form of these is represented by the solitary gland of the alimentary canal, which is nothing but a local hypertrophy of the indifferent element of the connective tissue of the part, and possesses no other capsule than that which necessarily results from its being surrounded by the latter. A number of such bodies as these, in contiguity, constitute, if they be developed within a mucous

membrane, a Peyer's patch; if within the walls of the splenic artery and its ramifications, a spleen; if within the walls of lymphatics, a lymphatic gland; if in the neighbourhood or within the substance (as in fishes) of the kidney, a supra-renal body; if in relation with a part of the brain, a pituitary body."*

So, according to Mr. Huxley, the tonsil is not a truly follicular gland, as is usually supposed; but is a "vascular gland" developed around a diverticulum of the pharyngeal mucous membrane, its "follicles" being constituted by imperfect septa of rudimentary connective tissue, containing a solid mass of indifferent tissue traversed by capillaries. And even the liver, so far as its parenchymatous portion is concerned, must be ranged (as Mr. Huxley has shown) in the same category; this part being very distinct, both homologically and functionally, from the excretory portion of the organ, as Dr. Handfield Jones's recent researches may, we think, be reasonably considered to prove.

"It seems odd," concludes Mr. Huxley, "that from being a sort of histological and physiological outcasts, the vascular glands should turn out, if this view be correct, to be the most important and extensive class of organs in the whole body, claiming *the* gland *par excellence*—the liver—as one of their family."

In the section on the *Lymphatics* of the spleen, we find nothing that we need stop to notice; Mr. Gray agreeing with all recent histologists in the statement, that the lymphatics of this organ present nothing which is in any respect peculiar, either as to size or distribution, and that they cannot be considered as performing the special function that was formerly assigned to them, of serving as the excretory duct for the removal of the products of its elaborating action. Neither is there anything novel in his description of the *Nerves* of this organ; and from the section on its *Comparative Anatomy*, we have already drawn the most important facts relating to its intimate structure; so that, although many very interesting details remain unnoticed, these are of a kind which have no direct bearing upon the principal object of the whole inquiry—namely, the *Function* of the spleen. To this, then, we shall now proceed; taking in connexion the sections on the *Blood* and on the *Physiology* of the spleen, which, in Mr. Gray's essay, are very widely, and somewhat unaccountably, separated, so as to involve a great amount of repetition that might easily have been spared.

The Blood of the Splenic Vein† has been examined by Mr. Gray, both microscopically and chemically; by the former method, in several hundred specimens from different animals; by the latter, in no fewer than a hundred and eleven samples obtained from eighty healthy horses.‡ It was compared with the arterial blood and with the ordinary venous blood of the same animals, or sometimes with the blood of the mesenteric vein;

* Quarterly Journal of Microscopical Science, vol. ii. p. 82.

† Mr. Gray means by this, *not* the blood drawn from the splenic vein during the continuance of the circulation, but that which has been expressed from the organ after its cessation. The latter, for reasons which will appear further on, may be far from furnishing a fair sample of the former: and we think that we shall succeed in showing that it must often be very different.

‡ We do not suppose Mr. Gray means to affirm that he has made 111 complete analyses of splenic blood, besides those of aortic and jugular blood, although his words (pp. 137 and 177) seem fairly to convey that idea. We cannot find that he has made more than eleven analyses of splenic blood, in which the amounts of its principal constituents have been determined; and the total 111 seems to include the analyses of other blood which were made for the sake of comparison, as well as all the partial analyses of splenic blood.

and the conditions of the observations were so varied, as to test the influence of *age*, the *period of the digestive process*, and the *general state of nutrition*, on the comparative amounts of their principal components. The general results—allowance being made for these modifying influences —were found to be so remarkably uniform, as apparently to leave no doubt whatever that they represent the general facts of the action of the spleen upon the blood which passes through it; and they seem also to be in precise conformity with the results of microscopical and chemical analysis of the components of the spleen itself; whilst the determination of the *conditions of variation* in the proportions of the principal constituents of the splenic blood, helps us to account for the differences in the analytical results obtained by previous observers.—Here again, however, we find ourselves called upon to take exception to the very loose and slovenly phraseology in which these conclusions are expressed. We have been continually obliged to fall back upon the tabular statements, in order to appreciate the real import of the terms in which their general results are summed up; a principal source of confusion (which we give as a sample of the whole) being that the word *amount* is used by itself in three different senses—namely, the *absolute amount* of each component, the *amount of its increase*, and the *amount of its diminution*—and this without any other indication than can be derived (and this not always with certainty) from the context.

The first point investigated was the *absolute quantity* of blood normally contained in the spleen, and the circumstances under which this undergoes variation. In 12 well-fed horses in which this was examined, at from 4 to 48 hours after a meal, the extremes were 724 and 2641 grains; the *maximum* being found about 16 hours after feeding, when the digestive process may be considered to have arrived at its completion; and the *minimum* at 48 hours, when the new material has probably been expended in the processes of nutrition, &c. In ill-fed, and still more in starved horses, the reduction in the quantity of blood contained in the spleen is most remarkable; for in one of the former, sixteen hours after taking food, there were only 371 grains, and in one of the latter, only 50 grains. As might be expected from the previous results, it was found that an increase of the entire bulk of the blood, by transfusion from another animal, produced a considerable augmentation of the amount of blood in the spleen; and that, on the other hand, by general loss of blood, that quantity underwent a special reduction. So, again, a temporary augmentation of the entire mass was found to take place after the ingestion of large quantities of fluid, especially when absorption proceeded rapidly. And any cause which obstructs the return of the blood through the splenic vein—such as disease of the liver, heart, or lungs, occasioning an impediment to the flow of the blood through either of those organs—produces an enormous increase in the amount of blood contained in the spleen: thus, a well-fed, healthy horse having been kept under the influence of chloroform for half an hour, during which time the respiratory movements were very imperfectly performed, its spleen was found to contain no less than 9000 grains of blood. That the vascular distensibility of the spleen varies greatly in different animals—being much greater in the Ruminants, for example, than in man—is a fact that has long been known; but Mr. Gray has

added much to our previous information upon this point; and one of the most interesting facts which he has noted in regard to the comparative anatomy of the organ as a whole, is its large size and great distensibility in *diving* animals, such as the seal and the ornithorhyncus, which are liable to have their respiratory circulation temporarily suspended, but which are not furnished with those arterial plexuses and multiplied venous reservoirs, that enable the cetacea to undergo a prolonged submersion without inconvenience. Thus it is obvious that one of the functions of the spleen is the regulation of the *quantity* of blood in circulation, since it acts as a *diverticulum* for the reception of a temporary excess in the whole bulk of the fluid, and also serves as a reservoir to relieve any accumulation, which may take place, from whatever cause, in the systemic venous circulation. There is no novelty in this doctrine; but Mr. Gray has furnished much valuable evidence in support of it. One mode of experimenting, however, does not seem to have occurred to him, which would have afforded unexceptionable information as to the degree of vascular distensibility of the spleen in different animals, and would, moreover, have probably furnished interesting results with regard to the conditions of the lacunar circulation—namely, to tie the splenic vein during life, and then to ascertain the amount of blood which the organ contained at different intervals subsequently to the operation, and to investigate the state of its parenchyma, both microscopically and chemically.

The peculiarities in the *microscopic* characters of the splenic blood are stated by Mr. Gray to be—1st. Considerable variations in the *size, form,* and *colour* of the red corpuscles, corresponding with those already described in the corpuscles which he considers to form part of the splenic parenchyma; 2nd. The *rare* or *occasional* presence of corpuscle-holding cells, corresponding with those met with in the spleen-pulp; 3rd. The *almost constant* existence of numerous pigment-granules, or masses, or rod-shaped crystals, these also existing either free, or contained in cells, as in the spleen-pulp; and 4th. The *constant existence* of a large number of *colourless corpuscles,* some of which are in the condition of nuclei, whilst others present more or less distinct traces (especially when treated with acetic acid) of an investing cell-wall; both kinds, however, being apparently derived directly from the splenic parenchyma. These peculiarities were constantly found to distinguish the blood of the splenic vein from that returning from any other organ; and no variations were discoverable by the microscope, either in the colour of the blood-discs, the number of coloured granules, or the proportion of the colourless corpuscles, according to the time that had elapsed since the ingestion of food.

The results of the *chemical* analyses performed by Mr. Gray (with the assistance of Dr. Noad) are tolerably satisfactory as to their general uniformity; at the same time that they present variations which can be frequently referred to certain modifying influences. It is a note-worthy deficiency in the account of these researches, that the mode of analysis adopted by Mr. Gray is not stated. It is well known to all who have attended to this subject, that the proportions of fibrin, corpuscles, albumen, and salts, that may be determined to exist in any given sample of blood, will vary greatly according to the method which has been followed; and there is perhaps none that can be relied on as giving absolutely true

results.* *Of course, where the same method has been employed throughout (as we presume to have been the case in Mr. Gray's researches)*, the results admit of mutual comparison; but they cannot be fairly compared with those of any other analyst, unless it is known that the like method has been followed by both.

The total amount of *solid matter* contained in the blood of the splenic vein is affirmed by Mr. Gray to be very considerably *less*, under ordinary circumstances, not only than that of arterial blood, but also than that of ordinary venous blood, which usually shows some diminution in this respect; this reduction, however, does not take place in all the components of the blood, being usually confined to the crassamentum, whilst the solids of the serum may even undergo an increase. When the question was still further narrowed by the separation of the several components of the blood, the very remarkable result was obtained, that the proportion of *red corpuscles* in splenic blood does not, under ordinary circumstances, average much above *one-half* of the proportion contained in arterial or in other venous blood. This reduction far exceeds that which presented itself in Béclard's analyses, the proportion of red corpuscles in splenic blood being stated by him as about *four-fifths* that of arterial blood, and about *nine-tenths* that of ordinary venous blood. The discrepancy is probably to be attributed, in part, to the mode in which the blood was obtained; but may be partly accounted for by differences in the conditions of the animals experimented on; for it was found by Mr. Gray, that the greatest reduction takes place at the time of the greatest turgescence of the spleen,—namely, about the period of completion of the digestive process, whilst the amount of change observed at an early period after the ingestion of food is very trifling; and in starved animals, *no change whatever* is detectible.

The proportion of *fibrin* contained in the emerging blood of the spleen is generally much larger than that contained either in arterial or in ordinary venous blood; the average of twelve experiments giving 6·4 parts per 1000, which was just *double* the average for the arterial blood, and *one-third more* than the average for the ordinary venous blood, of the same animals. Very marked variations presented themselves, however, the *maximum* being 11·53 per 1000, whilst the *minimum* was but 2·5 per 1000; and these variations had no perceptible relation to the period of the digestive process. Mr. Gray thinks that the increase of fibrin bears some relation to the diminution of the corpuscles, but his tables by no means bear out this notion; for although in the blood which presented the above-named maximum of fibrin, the corpuscles were reduced from 188·4 to 60·0, or less than *one-third*, another sample gave nearly the same proportion (10·88) of fibrin, with a far less reduction in the corpuscles; whilst another sample, with a reduction in the amount of corpuscles from 104·8 to 27·93, or not much more than *one-fourth*, yielded only 4·31 of fibrin. Mr. Gray states that the blood of ill-fed or starved horses always affords a very considerable proportion of fibrin, as compared with that of well-fed animals; but this assertion is by no means consistent with his own analyses.

The increase in the proportion of *albumen* in the splenic blood was by

* See the writer's Principles of Human Physiology, fourth edition, pp. 149—152.

no means so remarkable, especially as compared with ordinary venous blood: the average of ten analyses giving 60·0 parts per 1000, whilst in five of these animals the albumen of the arterial blood averaged 37·2 parts per 1000, and in four others the albumen of ordinary venous blood averaged 54·0 parts. The extremes, however, were much wider apart in the case of splenic blood; the *maximum* having been 83·3 parts, while the *minimum* was 35·3 parts. In this last case, which was the one in which the corpuscles were unaltered, there was an *absolute diminution* of the albumen, the arterial blood having contained 40·9 parts of that constituent. The conditions of this variation have not been made altogether clear by Mr. Gray's researches. He thinks that a marked increase in the amount of albumen is generally concurrent with a diminution in the amount of red corpuscles; but that this is not the case in the latest stages of the digestive process in well-fed animals, the corpuscles being then greatly reduced, while the albumen shows but a very trifling augmentation. The number of analyses appears to us far too small, however, and the individual discrepancies far too great, to admit of any such generalization.

The serum of splenic blood is stated by Mr. Gray to be distinguished by the presence of *colouring matters*, in sufficient quantity to impart a reddish-brown tinge to the residue left on evaporation, and to the various substances extracted from it; a fact which harmonizes with the previously-cited results of microscopic examination. He does not consider that the presence of neutral albuminate of soda, which is indicated by the formation of a whitish flocculent precipitate on the addition of about twenty times its own bulk of water, is at all peculiar (as Lehmann supposed) to splenic and hepatic blood; since he found a like deposit, in much greater quantity, in the serum of the arterial and of the jugular blood of the same animals.

The saline constituents of splenic blood do not show any marked peculiarity, either as to quantity or quality, save that the proportion of iron in the crassamentum is much greater than in either arterial or ordinary venous blood. Mr. Gray speaks of its *amount* as increased; but as the whole crassamentum is largely diminished, we apprehend that the increased percentage of iron does not do more than compensate for this reduction, probably not so much; for whilst, as it seems reasonable to suppose, the increased proportion of iron in the crassamentum is due to the dissolution of red corpuscles in the spleen, a part of the iron thus set free must be imparted to the serum through which the granules of altered hæmatin are diffused.*

Connecting the foregoing results of the examination of the *splenic blood* with the results derived from the microscopical and chemical examination of the spleen itself, Mr. Gray considers himself to be justified in drawing the following conclusions as to the influence exerted by this organ upon the *quality* of the blood which passes through it. First, as to the red corpuscles:

* Mr. Gray says (p. 357), that "the blood of the spleen contained in the greater majority of cases *a much larger quantity* of iron than was found either in the blood entering the gland, or in other venous blood;" but this statement is not justified by any of the analyses which he gives.

"The occasional, and in some animals the constant, occurrence of normal and changing blood-globules in the substance of the pulp, and their partial conversion into coloured pigment granules, or crystalline forms, the chemical analysis of which has shown to be identical with the hæmatin of the blood; the arrangement of the bloodvessels, as admirably adapted to admit of the occurrence of these changes under certain circumstances; the frequency of their occurrence throughout nearly the whole of the vertebrata,—all these facts, I think, are in exact harmony with the results of the analyses of the splenic blood, as far as the diminution of the blood-globules is concerned. They clearly show that, in all animals, under certain circumstances, the spleen modifies the constituents of the blood-discs during their transit through the organ, retaining them for a time in the pulp tissue, and changing the elements of which they are composed." (p. 356.)

It is, however, only when the vascular turgescence of the spleen is such as to cause an extravasation of blood into its pulp, that the corpuscles are thus melted-down: and thus it is that the process is specially observed to take place as a normal occurrence in well-fed animals, at the time when the digestion of a recent meal is introducing a fresh supply of solid matter into the circulation; and that the blood returns from the spleen with little or no change in this respect, in animals whose blood has been impoverished by starvation. Mr. Gray's hypothesis with regard to the alteration in the proportion of *iron*, which he considers that the spleen effects in the blood in its passage through it, is vitiated by the uncertainty which attaches to the assumptions on which he bases it. He supposes that the red pulp of the spleen withdraws iron from the blood at certain epochs, to reimpart it to the circulating fluid at other times; and that the *unaltered* blood-discs of the emerging blood receive an extra charge of this component. The large proportion of iron in the crassamentum of splenic blood, however, seems to us to be readily accounted for by the fact, that this crassamentum does not consist of normal corpuscles alone, but that it is made up of blood-discs in all stages of degeneration, which retain their hæmatin, whilst they part with their other contents; so that an unusually large proportion of hæmatin and its metamorphic derivations will naturally be present, part of it still within cells, but another part probably diffused in the condition of pigment granules. A proportional excess of iron in the crassamentum, moreover, is far from indicating (as we have already pointed out) an absolute excess of iron in the whole mass of splenic blood; so that on neither point do we consider that Mr. Gray's hypothesis can be considered as otherwise than purely speculative.

Mr. Gray considers that the facts which he has collected warrant the conclusion, that the colourless elements of the spleen-pulp and of the Malpighian bodies serve as a sinking-fund for *albuminous* materials, during those conditions of the system in which the supply exceeds the demand; a development of cells and nuclei taking place at the expense of the surplus albumen of the blood during the later stages of the digestive process; and a deliquescence of these lowly-organized forms of tissue occurring in the intervals, whereby the proportion of albumen in the blood which passes through the spleen is augmented. He further supposes that part of the increased amount of albumen usually found in splenic blood, may be derived from the colourless components of the red corpuscles which have undergone disintegration in the organ. To this

source, rather than to any direct change effected by the tissues of the spleen in the blood which passes through it, he is inclined to refer the increase in the proportion of *fibrin* usually presented by splenic blood; but, as already remarked, there is an entire want of conformity between the two classes of facts, as shown in Mr. Gray's own table. Thus in Table II., No. 10 (p. 157), we find the fibrin raised from 0·78 (arterial) to 10·3 (splenic), whilst the corpuscles exhibited *no diminution;* whilst in No. 8, in which the corpuscles were reduced from 104·80 (arterial) to 27·93 (splenic), the proportion of fibrin was only raised from 1·79 to 4·31.—With regard to the function attributed to the spleen by Professor Kölliker, of specially preparing pigmentary matter for the bile, by the disintegration of blood-corpuscles, Mr. Gray shows that the idea derives no support from fact; since (1) there is no special chemical relation between the colouring matter of the splenic blood and the pigment of bile; (2) the removal of the spleen in animals does not affect the colouring matter of the bile, which is as abundant after as before the operation; whilst (3) the bile-pigment is generated before the development of the splenic vein in the chick. He does not deny, however, that part of the free colouring-matter of the splenic blood may be changed into bile-pigment; though this, for the reasons just stated, cannot be the special object of the disintegration of blood-corpuscles in the tissue of the spleen, as Kölliker supposed.

Thus, according to Mr. Gray, the Spleen serves to balance alike the *quantity* and the *quality* of the blood; and is specially adapted for this function by its connexion with that part of the vascular system which is concerned in the introduction of new material into the circulation. This function, however, is so far from being absolutely essential to life, that the complete removal of the organ does not seem to be attended with any injurious consequences; as Mr. Gray has ascertained by his own experiments, which confirm those of many previous inquirers. He does not speak of having observed any symptoms of plethora, such as those noticed by Dobson; but he tells us that two of the cats on which he operated, having been kept for a considerable time, improved much in condition, growing to a much larger size than other uninjured cats of the same age. And he supposes that the function of the spleen, in so far as this consists in the disintegration of the red corpuscles, and in the increase of the albumen of the blood, is taken on by other parts and organs; since there is usually found to be a slight difference in both these particulars between arterial and ordinary venous blood; while the general hypertrophy of the tissues just noticed as an occasional result of the removal of the spleen, would seem to show that the augmented amount of albumen is stored up in the system generally, when it cannot be received by the spleen.

"Although these elements," he remarks, "cannot as readily, under the above conditions, be restored to the blood, as if the spleen had retained them, to be used at every occasion or requirement of the animal, still their removal from this fluid, where they exist in great excess, serves to effectually prevent the inconvenience which a too great accumulation of them in the blood would certainly occasion." (p. 372.)

Fully admitting the force of all the facts upon which Mr. Gray has attempted to build up his physiological induction, we must yet confess

that he does not appear to us to have by any means succeeded in affording a complete solution of this obscure but interesting problem. There is a difficulty which does not seem to have presented itself to him, arising out of the very magnitude of those alterations in the constituents of the blood, of which he affirms that the spleen is the instrument. In Table IX. (p. 177), which gives the average results of 111 analyses of the aortic, jugular, and splenic blood of the horse, we find the following to be the amounts of water and of solid constituents in each respectively:

	Aortic.		Jugular.		Splenic.
Water . . .	789·14	...	793·42	...	829·81
Solid matter .	210·86	...	206·58	...	170·19

Thus, whilst arterial blood undergoes a trifling reduction in the amount of its solids, during its passage through the capillaries of the general system, the blood which is transmitted to the spleen loses, on the average, 40·67 parts, or *nearly one-fifth.* And since, in many of the experiments, the loss was very trifling, it must have been far more than this in a large number of instances, as, for example, in No. 8 of Table II., in which the albumen, fibrin, and red corpuscles of aortic blood amounted to 145·85, whilst the same components of the splenic blood amounted only to 73·47; the difference thus being 72·38, or nearly *one-half.* Now the question which we affirm to be altogether unsolved by Mr. Gray's researches, and to be altogether passed by in his physiological conclusions, is twofold:—1. What becomes of all the solid matter of the blood, which is thus apparently kept back by the spleen?—and 2. How is it that this reduction in the amount of solid matter does not produce a greater effect upon the general mass of the solid constituents of the blood? We know well, that *at the ordinary rate of the circulation,* the whole amount of blood in the spleen must be renewed *many times in the course of a single minute;* and all the blood of the body must be submitted to the same operation *many times in an hour.* If, then, the spleen keep back one-fifth of the solid contents of every pound of blood that passes through it, the organ must soon draw into itself nearly the whole mass of albumen, fibrin, and corpuscles contained in the circulating current; and the blood must be impoverished in a corresponding degree. Our readers will easily apprehend our meaning, if they will call to mind the process by which a large receiver may have its air exhausted by a very small pump, provided that the working of this pump be kept up for a sufficient length of time. Now in this case, all the air which has been drawn from the receiver by the pump, is discharged above the piston as fast as it enters beneath; and consequently there is no accumulation. But in the case of the spleen, there is no such outlet; no means can be traced whereby the withdrawn materials can be disposed of through any other channel; and we seem reduced to the inference, that they are solely, or at least chiefly, applied to the augmentation of the substance of the organ itself. But although this does undergo a certain increase, the amount of such increase is as nothing, compared to that which must result from the retention of one-fifth of all the solid matter of the blood flowing through the spleen at the ordinary rate. And, conversely, the amount of reduction in the solid constituents of the whole mass of blood, by the instrumentality of the

spleen, is so small as to be inappreciable; instead of immediately manifesting itself, as it must have done, if the current flowing through the spleen at the ordinary rate, even for a limited time, had suffered a deprivation of one-fifth of its solid constituents. We cannot ourselves perceive any other escape from this dilemma, than by the assumption,—which we admit to be purely hypothetical, but which seems to us to be the only hypothesis that will at all meet the facts of the case,—that a large part of the current of blood through the spleen is retarded, almost stagnated, at the time when the organ is in fullest action. And if we seek for a cause for such stagnation, we think that one may possibly be found in the increase of pressure within the tributaries of the vena portæ, arising from the absorption of a large quantity of new alimentary material into the mesenteric vein. If that pressure should so augment as to antagonise (or nearly so) the pressure within the splenic artery, a stagnation of the blood-current in the venous reservoirs of the spleen will be the consequence; and the increased pressure will, at the same time, give rise to the escape of red corpuscles from the lacunar system of the spleen into its parenchyma, which Mr. Gray affirms to be a normal phenomenon in well-fed animals. The well-known fact that the splenic vein is destitute of valves, seems to us to add some weight to this hypothesis; since an excess of *vis à fronte* over the *vis à tergo* may cause not only a stagnation of blood in the veins of the spleen, but may even permit a reflux into them from other parts of the portal system. And thus it may happen, that the whole mass of blood obtained from the spleen after death, may differ widely in composition from the blood which flows back from the splenic vein; for it would include the portion which has been long stagnant in the spleen, as well as that which the organ has just before received from the arteries; it being only, in fact, when no such stagnation occurs, that the one will properly represent the other. It is quite conceivable, moreover, that even the blood drawn from the splenic vein during life, might not represent fairly that which is normally returning from the organ; since the relief of the backward pressure afforded by the free emission of fluid, will naturally allow a portion of the stagnant blood of the spleen to flow onwards, and thus to alter the character of the discharged sample.

We offer these suggestions as the only helps that occur to us towards a solution of the difficulty in question. One other possible fallacy, however, seems to vitiate the averages of Mr. Gray's analyses. It is quite clear that whatever the spleen *withdraws* from the blood, it must have *given back* the same amount by the time that it returns to its previous dimensions; and it seems to us likely that, whilst the period of withdrawal may be brief, the period of restoration may be long, or *vice versâ*. The former may probably be the case with the red corpuscles, which are withdrawn by extravasation, whilst their contents are given back by slow disintegration. The latter may be the case with respect to albumen, whose appropriation by the process of cell-growth may be slow, whilst its return to the blood, by the bursting or liquefaction of its containing tissues, may be rapid. Thus, in any small number of analyses, the disappearance of corpuscles and the increase of albumen would be the ostensible change; whilst a series made upon a sufficiently long succession of samples

of the blood returning from the spleen, might show that this was antagonised by a converse operation, less in amount, but distributed over a longer period. At any rate, we may safely demand that, before any physiological conclusions whatever be drawn from such tables as Mr. Gray's, the fact shall be accounted for, that, notwithstanding the organ constantly retains in its substance as much as one-fifth (on the average) of the solid matter of all the blood passing through it, it does not undergo more than a limited and occasional increase in dimensions, returning in the intervals to its usual size, while the general mass of the blood is not affected in any appreciable degree by this withdrawal of its most important materials.

Mr. Gray's hypothesis of the mode in which the alteration in the proportions of the principal constituents is effected, remains to be considered. The amounts of these contained in the aortic, jugular, and splenic blood of the same animals, are given in some detail by Mr. Gray, in Table IX.; for our purpose, however, the following summary of averages will suffice:

	Aortic.	Jugular.	Splenic.	Difference between aortic and splenic.
Fibrin	2·30 ...	4·20 ...	6·54 ...	+4·24
Corpuscles	157·20 ...	136·80 ...	88·58 ...	—68·62
Albumen	42·00 ...	54·40 ...	63·00 ...	+21·00
Saline, fatty, and extractive matters	9·36 ...	11·18 ...	12·07 ...	+2·71
Total solids	210·86 ...	206·58 ...	170·19	

Now, if the amounts of albumen, fibrin, and extractive, which present themselves in excess in splenic blood, as compared with aortic, bore any kind of correspondence to the amount of corpuscles which disappear, there might be some ground for supposing the addition of the former to be the result of the disintegration of the latter. But such, on Mr. Gray's own showing, is so far from being the case, that their united increase amounts to no more than 27·95 parts, whilst the diminution in the corpuscles is 68·62. Besides, there appears to us to be strong evidence that the two sets of phenomena have no direct relation, one to the other: for, as we have already remarked, a comparison of the results of individual analyses shows no constant correspondence between them; and besides, although the changes which take place in the colourless portion of the splenic parenchyma might be conceived to be influenced by those occurring in the coloured portion with which it is so intimately intermingled, it is scarcely conceivable that the changes in the interior of the Malpighian bodies (with which, as argued by Mr. Gray himself, those of the colourless parenchyma correspond) are in any way affected by the disintegration of blood corpuscles; more especially since comparative anatomy shows us that the colourless parenchyma may perform its functions without any diffusion of red corpuscles through its substance. We are inclined to believe, therefore, that the office of the *colourless* parenchyma of the spleen is not only to serve as a storehouse for the surplus albumen that finds its way into the circulation on the completion of the digestive process, but also to exert an *assimilating* action upon it, whereby it is rendered more fit for the nutrition of the tissues; and of this assimilating action, we deem the generation of fibrin to be one of the results. And if it be true,

as we have elsewhere suggested,* that one special function of the red corpuscles is to assimilate or prepare that peculiar combination of materials which is required for the nutrition of the nervo-muscular apparatus, the disintegration of these corpuscles in the splenic parenchyma may answer the two-fold purpose of regulating their total proportion in the mass of the blood, and of diffusing through the liquor sanguinis the materials which the nervous and muscular tissues are to draw from it for their own development.

We have thus endeavoured to present our readers with a fair account of Mr. Gray's researches; to show what are the points which they may be considered to have established; to state with explicitness the problems which yet remain unsolved; and to indicate the directions wherein, as it seems to us, the elucidation of these may be sought with the greatest prospect of success. We trust that we shall not be thought to have undervalued Mr. Gray's labours, because we hold some of his conclusions to be less definite and unobjectionable than he has himself supposed them to be. On the contrary, it is in the very suggestion of yet higher questions than those which he believed himself to have answered, that much of their merit in our eyes consists; and we trust that he will not consider his work accomplished, until he has attained to a satisfactory solution of these, and of others yet beyond, which may suggest themselves in the course of his further researches.

William B. Carpenter.

REVIEW II.

Lettsomian Lectures on Insanity. By FORBES WINSLOW, M.D., D.C.L., late President of the Medical Society of London.—*London,* 1854. 8vo. pp. 160.

WHEN the London Medical and Westminster Societies were amalgamated in 1850, the council, in order to honour the memory of Dr. Lettsom, the founder and benefactor of the parent institution, established two lectureships, to be held annually by a physician and surgeon. These are the Lettsomian Professors of Medicine and Surgery. Dr. Owen Rees was nominated to be the first occupant of the chair of medicine, and Dr. Forbes Winslow was elected to be the second. It was natural to expect that Dr. Winslow would direct the attention of the Society to the subject of insanity. From an early period of his professional studies, he has investigated mental phenomena theoretically, and treated the aberrant forms of mental action curatively; and as the active and enterprising founder and editor of the 'Journal of Psychological Medicine' has manifested a warm interest in the advancement of mental pathology. Dr. Winslow has arranged the results of his researches and experience under three heads, constituting three lectures. The first is entitled 'The Psychological Vocation of the Physician;' its objects are to demonstrate the advantages to the theory and practice of medicine which flow from a more general and accurate knowledge of the science of mind on the part of the physician, to establish the close connexion between the two sciences,

* Principles of Human Physiology, fourth edition, p. 187.

and to illustrate the true philosophic character of the medical practitioner. The second lecture is 'On the Medical Treatment of Insanity;' and the third 'On Medico-legal Evidence in Cases of Insanity.' The two latter are therefore especially practical, the first is more nearly related to medical deontology. In it the applications of mental philosophy and logic to the daily routine of medical practice are demonstrated, and the advantages which the wise physician can confer on his patients by *moral* remedies are set forth. Hope, ease of mind, and the calm influences of religion, are powerful means of action on the organization, and are in the hands of the medical practitioner to a large extent. When all human means fail—and sooner or later they certainly must fail—to ward off the most dreaded termination of sickness, when nothing more remains to be done than to "await the inevitable hour," it is still within the psychological vocation of the physician, alike to soothe the parting moments of the dying, and make the solemn event a useful lesson to the survivors. Again, it happens from time to time that the influences of religion and moral training, and the consolations which the Christian derives from his faith, are barren and unfelt, from certain morbid conditions of the corporeal functions. In such cases it is of vast importance that the physician be able to detect these conditions, and learn their exact relation to that bondage in which the soul is held; otherwise crime and despair may overwhelm it. Illustrations of these views are given, and the true psychological vocation of the physician in this respect is fully shown. The following quotation will illustrate Dr. Winslow's enlarged views and eloquent diction:

"Finally, I would observe, that of all the subjects that can occupy the attention of the philosophic physician, none equals in importance or in grandeur those which I have had the honour of recommending to your special attention. What can compare in dignity, in sublimity, in comprehensiveness, or in the lofty aim of its disquisitions, to the study of the nature and operation of that spiritual essence, upon the right knowledge and cultivation of which depends our happiness, both in time and in eternity? As the mind advances in a knowledge of its own phenomena, the intellect expands, new sources of delight open to us, and the pleasure we experience in the pursuit of these exalted speculations impresses forcibly upon the mind itself conclusive evidence of its own DIVINITY He who has habituated himself to trace out the numerous applications of mental philosophy to the important subjects of education, morals, and legislation; to analyse the nature of thought, the laws regulating the association of our ideas, the springs of action, the origin of our happiness, the laws of moral science, the nature of the passions, the formation of character, the foundation of our hopes, and the influence of our emotions,—will appreciate the value of this branch of science. The physician will be conscious, as he advances in a knowledge of the constitution of the mind, that his love of truth is growing strong; and whilst, in the spirit of true humility, he acknowledges the limited nature of his intellectual powers, he will, whilst contemplating their grandeur and importance, recognise the GOODNESS AND MAJESTY OF GOD." (p. 44.)

The *second* lecture is a clear and concise epitome of the *medical* treatment of insanity, as distinguished from the *moral*. We believe that not a few will concur with Dr. Winslow in the expression of his regret for the neglect of physical remedial agents by some of those practitioners who possess great and special opportunities for observation and practice. He attributes it principally to "the doctrine promulgated by writers of celebrity—

by men referred to and reverenced as our authorities and guides in their special department of medicine, that for the cure of insanity moral treatment is entitled to the highest rank," and thus the administration of remedies suitable to the morbid corporeal states has been discountenanced, or rendered a secondary consideration. Dr. Winslow quotes one recent writer as remarking "'when one man thinks himself a king, another a cobbler, and another that he can govern the world with his little finger, *can physic make him think otherwise?*'" Another writer of the same school observes, "'To prescribe for the mad, whilst *its nature remains a mystery*, is to prescribe for a *phantom!*'"

Another cause, well indicated by Dr. Winslow, is the unphilosophical nature of the hypotheses which have been broached with the view of explaining the phenomena of insanity. In the early periods of the history of medicine (as it is even now in some countries), it was attributed to a Divine afflatus, to the Divine wrath, to demoniacal, Satanic, or malignant influence. These views, "in a modified, less offensive, and different form," still hold their ground in the convictions of many, and constitute the basis of the belief that insanity is an affection of the immaterial principle. Dr. Winslow argues eloquently and, we think, very conclusively in favour of the doctrine that it is simply a *disease of the brain:*

"When we assert that the 'functional' or 'spiritual' theory will not bear the test of serious examination—that it is at variance with all *à priori* and *à posteriori* reasoning—that it stands in direct opposition to positive, well-recognised, undeniable data, we are met by the interrogatory, Can you demonstrate to us the specific character of the change induced in the nervous matter, which it is alleged gives rise to mental derangement? and do not the scalpel and the microscope of the morbid anatomist in vain endeavour to ascertain in many cases of positive, violent, and unequivocal insanity, any appreciable structural lesion in the nervous matter, in its investing membranes, or organs in close association with the brain, sufficient to account satisfactorily for the morbid phenomena exhibited during life? One would really infer, from the reasoning and assertions of those who take these spiritual views, and who repudiate the idea of insanity ever being the result of physical change in the condition of some portion of the brain or its appendages, that the encephalon has no specific functions allotted to it; that it is altogether a useless and supernumerary organ; that it was created for no wise purposes; and that, as far as the phenomena of mind were concerned, we could have done as well without as with the brain! If this organ be not the material instrument of mind—if it be not the medium through which the spiritual portion of our nature manifests its powers—the centre of sensation—the source of volition—the seat of the passions—

'The dome of thought,—the palace of the soul'—

I ask, what *are* its functions, its specific uses and operations?—for what object was this most exquisitely organized and complicated structure formed?—why does it receive so large a proportion of the blood, and why is it so carefully protected from injury? These interrogatories naturally arise in the mind, when we hear so unphilosophical and so unphysiological a theory propounded with reference to the possibility of the mind being *subject to disease apart from all derangement of the material organs with which it is so closely and indissolubly associated.* Can we conceive a more preposterous notion than that sanctioned by high authority, and which inculcates that the spiritual principle admits of being distorted, deluded, depressed, exaggerated, perverted, exalted, independently of any form of bodily disease, or modification of nervous matter?" (p. 52.)

The anatomical objection appears always, at a first glance, to be the

most insuperable, but a more mature consideration of the subject will lead to the conclusion, that structural change so important and extensive as to be *easily* discoverable by the anatomist, cannot be reasonably expected to be present in the cerebra of the insane, so long as the mental phenomena are simply *disordered* and not abolished. A certain integrity of *structure* is necessary for the manifestation of even the phenomena of insanity. The phenomena themselves, however, offer the most conclusive data in favour of the cerebral origin of the disease, and the practitioner whose sphere of labour brings him into constant intercourse with the insane would, it might be supposed, fully corroborate these views; but such is not the fact. It is not, therefore, surprising that laymen should oppose the *cerebral* as opposed to the *metaphysical* theory of insanity. Yet no man of common sense, whether practitioner or not, doubts that he thinks with his brain, or that in insanity the brain is affected; and no metaphysician, except a solitary eccentric thinker, will deny the undeniable principle that the brain is the organ of the mind. This singular discrepancy between the premises and the necessary conclusion is of great importance in mental philosophy and pathology, and deserves a more special examination. We believe it is wholly due to the want of a clear and satisfactory system of cerebral physiology. The metaphysician does not possess such a system, nor does he seek to establish it; but the metaphysician guides public opinion, and the general public, with the metaphysician, therefore, ignores cerebral physiology. We put this question one day to a profound and lucid thinker of this class: Has mental philosophy or metaphysics contributed anything to the elucidation of the nature and cure of insanity, or of other aberrant forms of mental action now prevalent? His answer necessarily was, that neither had contributed anything. The writer of an able essay on Locke's Character and Philosophy, in a recent number of the 'Edinburgh Review,' happily illustrates the reason why metaphysical speculations are so sterile *quoad* medical art, and so devoid of all practical application to the needs of everyday life. He remarks:

"The metaphysician, above almost any other thinker, must . . . . draw from his own resources; patient excogitation must be his great instrument. Indeed, all great thinkers will rather delight in this than in mere acquisition; it ever has been, and ever will be, their *characteristic*. But, then, to be safe, such self-reliance must be accompanied with a careful survey of what has been done by others in the same field."*

Thus one-half the requisite for inductive inquiry is omitted—namely, *patient observation;* and one-half the object to be observed is left out—namely, the cerebrum and its mode of action, as the organ of thought. The "patient excogitation," which this able writer indicates as the great instrument of metaphysical research, has been wielded in Germany by the (perhaps) greatest intellects that have appeared in any age, and yet what has been the result? In this same article we find that Sir James Mackintosh amusingly characterized this German philosophy as "accursed," and Victor Cousin as "detestable!" We are sure all who, like Sir James Mackintosh, have "endeavoured to master" it, will agree with the conclusion regarding it at which the Edinburgh reviewer has arrived. He likens the inquirer to a lover of the picturesque in a mountain region,

* The Edinburgh Review, No. 202 (April, 1854), p. 391.

tempted to ascend some unknown peak on which the clouds still rest, assured they will shortly clear off. The early progress is delightful, as he glances back from time to time over the ample valley radiant in the sunlight; but when he has got into the loftier regions, and approaches the object of his aspiring ambition, circumstances are changed:

"He can see nothing but a rolling cloud of vapour, which hides every object ten inches from his nose, and after standing wetted to the skin, and shivering in 'darkness visible' for a couple of hours or so, in which the envious clouds still envelope him—now and then teased, perhaps, by a momentary rent in the veil, which seems to show him something, but too transiently to let him know what—he descends, and is glad to catch a glimpse of things in sunlight again. But for any purpose of pleasure or knowledge in ascending these cloudy regions, he might as well have sat himself down at the base of the mountain, and drawn a thick cotton night-cap over his head."

Such, we are assured by the reviewer, is the general estimate both in England and on the Continent, "of the philosophical value of a vast deal of German philosophy since Kant's time." Patient excogitation has here, then, been worse than useless.

Various reasons may be alleged for the entire neglect of cerebral physiology by professed metaphysicians. Firstly, a thorough *practical* knowledge of neurology is necessary to its investigation; secondly, keen powers of observation. Either of these is rarely possessed by speculative thinkers; very rarely indeed are they combined in one mind. The powerful thinkers, of whom "patient *excogitation*" is a special characteristic, are too much abstracted from the common affairs of life to give that practical turn to their studies which is required to make them useful to curative art; and have too little taste for that minute and incessant *observation* which is necessary to make the accomplished neurologist or cerebral physiologist. They, therefore, almost with one consent, leave this important half of their subject to the physician and physiologist, and turn to them for guidance and instruction. A moral cowardice and deeply-rooted prejudices have influenced others. Mental philosophy has been cultivated mainly with reference to theology and morals, and it has been a constant object to harmonize its principles with the dogmas of the one and the teachings of the other. Now the study of organization in reference to these has been falsely termed materialism, and the *materialist* has ever been viewed by the theologian as little better than an avowed unbeliever. Religious teaching, therefore, and the dread of being stigmatised as heterodox, have both raised an almost insuperable obstacle to the study of mental philosophy, in relation to organization, by those whose special vocation such study ought to be.

It cannot be denied, we think, on the other hand, that professional views as to cerebral physiology have been deservedly neglected. With one exception—phrenology—they have never been systematized or arranged so as to be attractive or even intelligible to the metaphysician. They have too often repelled, indeed, by their crudeness, by the total absence of the metaphysical element, or by the incorporation with them of a real naked materialism. To *phrenology* cerebral physiology owes much: to numerous *phrenologists* it owes little. The great and fundamental doctrines of a sound and *practical* mental philosophy must be the

fundamental doctrines of phrenology—namely, that the brain is the organ of the mind, and that it is double and multiple. Subordinate doctrines of phrenology must also be incorporated with such a system of mental philosophy as the practitioner desires; but we fear phrenology itself—*as a system*—is irretrievably damaged by its friends. On the one hand, the presuming empiricism which has been grafted upon it, chiefly by its illiterate cultivators, repels the thoughtful and cautious inquirer; on the other, the avowed materialism of another section of phrenologists has shocked those religious convictions which are deeply seated in man's nature, and are based upon the grandest doctrine of theology—the immortality of the soul. Nor is it possible for the impartial inquirer, rigorously divesting himself of all theological prepossessions (if such they may be termed), to avoid the conclusion, that the naked materialism which declares mind has no existence, is alike opposed to the conclusions of inductive inquiry and of common sense. The decline of phrenology may be, we think, dated from June, 1842, when Dr. Engledue delivered, at Exeter Hall, the introductory address, to a general meeting of the Phrenological Association. In that address Dr. Engledue proposed to substitute the word *cerebration* for mind, which he declared to be non-existent. All the leading phrenologists strenuously repudiated this doctrine, and a large number withdrew from the association. These steps did not, however, arrest the evil consequences anticipated from Dr. Engledue's avowal, but perhaps rather hurried them on, for phrenology became more and more popular in connexion with zoism, phreno-mesmerism, &c., and more and more abhorrent to the inductive philosopher, the metaphysician, and the physiologist.

The imperfections of mental philosophy and of cerebral physiology which we have indicated, are eminently of a nature to puzzle the practitioner. Both sciences develop great truths, and yet they seem to be antagonistic; the truths are thus neutralized, and a practical application is impossible.

So long as, in metaphysical inquiries, the existence of the material organ is practically ignored, so long will the state of that material organ be practically ignored also, in those forms of chronic disease of the cerebrum involving the judgment and the morals. Furious mania, dementia, and idiotcy, are easily connected, both theoretically and practically, with cerebral disease, by both metaphysician and practitioner. It is "*rational*" insanity which is referred with the greatest difficulty to structural disorder, and most easily comprehended as a disease of the mind distinct from organization.

These imperfections undoubtedly constitute the reason why the treatment of the insane has been so contradictory and empirical. It cannot be denied, however, that modern neurology (including cerebral physiology), has very considerably advanced the treatment of mental disorder. For a long series of ages mental philosophy had undivided sway; for an equally long series of years the insane were the victims of cruelties innumerable; and not the insane only, but all that large class of persons of aberrant or weak intellect to whom the mysterious has irresistible charms. In every past age, all kinds of enthusiasts (now left harmlessly to their vagaries), were very commonly the subjects of violence and per-

secution. As to these, cerebral physiology has *indirectly* done much. Were it not for the general enlightenment it has afforded, the spirit-rappers, magnetizers, electro-biologists, and the rest, might experience something more serious from the world than a smile at their busy credulity.

Another important consideration arising out of the hypotheses as to the nature of insanity, is the question of its *curability*. Dr. Winslow very happily shows the importance of a sound theory in this respect. He remarks:

"No right estimate can be entertained of the importance of these investigations unless we apply to the study of the diseases of the brain, and the cure of its disorders, the same enlarged and general principles which guide us in the investigation and treatment of the affections of other organic structures. An error of some magnitude has been committed by those who consider insanity to be a special, uniform, specific, and peculiar malady, justifying us in placing those so afflicted out of the ordinary nosological scale and sphere of medical practice. Again, it is necessary that we should, before being able to appreciate the effect of medical treatment, entertain just and enlightened views as to the CURABILITY OF INSANITY. I now speak from a somewhat enlarged experience, from much anxious consideration of the matter, and I have no hesitation in affirming that, if brought within the sphere of medical treatment in the earlier stages, or even within a few months of the attack, insanity, unless the result of severe physical injury to the head, or connected with a peculiar conformation of chest and cranium, and an hereditary diathesis, *is as easily curable as any other form of bodily disease for the treatment of which we apply the resources of our art.* Can there be a more lamentable error, or a more dangerous, false, or unhappy doctrine than that urged by those who maintain that the disordered affections of the mind are not amenable to the recognised principles of medical science? . . . . The existence of so vast an amount of incurable insanity within the wards of our national and private asylums, is a fact pregnant with important truths. In the history of these unhappy persons—these lost and ruined minds—we read, in many cases, recorded the sad, melancholy, and lamentable results of either a total neglect of all efficient curative treatment at a period when it might have arrested the onward advance of the cerebral mischief, and maintained reason upon her seat; or of the use of injudicious and unjustifiable measures of treatment under mistaken notions of the nature and pathology of the disease. . . . . My experience irresistibly leads to the conclusion that we have often in our power the means of curing insanity, even after it has been of some years' duration, *if we obtain a thorough appreciation of the physical and mental aspects of the case, and perseveringly and continuously apply remedial measures for its removal.*" (pp. 59—61.)

It has already been abundantly proved how much may be done in cases of congenital idiotcy, hitherto considered to be hopelessly incurable; in analogous cases of so-called incurable cases of insanity, it will sooner or later be shown (we feel confident) that the resources of the medical art have been under estimated.

It being considered to be an established proposition, that insanity is nothing else than a cerebral disease, we have to inquire what is its nature and seat? Every man who has reflected on the phenomena of his own mind—not as a metaphysician only, but as an observing physiologist—must have noted daily what a varied number of physical or corporeal influences change the action of his mind. As to some of the changes thus induced, he can clearly see, also, that if they were more intense and more prolonged—that is to say, different only in extent and duration,

they would constitute insanity. But they are evanescent, enduring only for the shortest period, and quickly ending in normal, healthy activity of the cerebrum. Some forms of insanity are, indeed, nothing else than transient, momentary states of this kind prolonged during a sufficiently lengthened period, so as to influence the actions of the individual to a degree which renders him no longer a free and responsible agent. Such attacks are, however, so evanescent, that it is obvious no important change in the material organ has occurred. Not so, however, with the persistent forms. As to these, we think Dr. Winslow's views are reasonable, and in accordance with experience. He believes them—

"To be the result of a *specific morbid action of the hemispherical ganglia, ranging from irritation, passive and active congestion, up to positive and unmistakeable inflammatory action.* This state of the brain may be confined to one or two of the six layers composing the hemispherical ganglia; but all the layers are generally more or less implicated, in conjunction with the tubular fibres passing from the hemispheres through the vesicular neurine. This specific inflammation, from its incipient to the more advanced stage, is often associated with great vital and nervous depression." (p. 61.)

Conjoined with this condition of the hemispherical ganglia, there may be general congestion or inflammatory action of the brain or its membranes; all those manifest structural changes in these tissues found after death, are, without exception, in Dr. Winslow's opinion, "the results or sequelæ of that specific inflammatory condition of the hemispherical ganglia" which he indicates. That this is true of a very large proportion of cases will be generally allowed. There are some forms of insanity, however, which can hardly be classed with them—namely, those in which there are simply fixed erroneous ideas. We have in the "electro-biologized" a transient, in fanatics and enthusiasts a permanent, form of this kind of cerebral change. In these, it would appear, as if the usual healthy action of the cerebrum *quoad* the erroneous ideas, had been just so much changed as to fix them in such a way, that whenever the succession of ideas passing through the mind brought these within the chain, they were presented to the consciousness, and not the ideas which would normally have arisen, had the cerebral tissue been free to act. It is apparent that a very minute pathological change in the cerebrum need only be assumed in these cases of fixed ideas, and this being granted, it would be difficult to assign it to inflammatory action, or even congestion.

The principles of *medical treatment* laid down by Dr. Winslow, flow from the pathological premises. There is no uniform method of treatment. In proportion as the symptoms approach to those of phrenitis or meningitis, in other words, in proportion as they indicate active inflammatory action, general and local blood-letting is advisable. Local bleeding is not to be confined to the seat of the disease, for it not unfrequently happens that it may be adopted with reference to a distant viscus. Leeches to the vulva and thighs are beneficial in cases concurrent with the menstrual period; to the sphincter ani, in those obviously connected with suppressed hæmorrhoids. In some instances Dr. Winslow has applied leeches to the Schneiderian membrane, particularly in those cases occurring in early life, and in persons of plethoric constitution and of sanguine temperament. He has known illusions of hearing or of vision which had embittered the

patient's life, removed by leeching behind the ears, or over the superciliary ridges. Dr. Winslow also speaks in warm terms as to the great utility, in acute mania, of prolonged hot baths, according to the method of Dr. Brierre de Boismont, of Paris. The patients remain from eight to fifteen hours in warm baths, at 82° to 86° Fahr., whilst a current of water at 60° is continually poured over the head. The method is inadmissible in intermittent mania, and in insanity beginning with great mental derangement, or associated with epilepsy or general paralysis.*

The class of *sedatives*, or in other words agents, modifying directly the condition of the cerebral tissue, constitute very valuable remedies. In recent acute cases they are generally admissible; it is in the various chronic forms of melancholia they are most useful. Dr. Winslow observes,

"In suicidal insanity, when local cerebral congestion is absent, and the general health and secretions are in good condition, the aconite and hydrochlorate of morphia often act like a charm, if *uninterruptedly and perseveringly given* until the nervous system is completely under their influence. . . . . I could cite the particulars of numerous cases of this form of insanity radically cured by the occasional local abstraction of blood from the head, the administration of alteratives, the warm bath, and sedatives. In the exhibition of this powerful curative agent, our success will often depend upon a *ready adaptation of the form of sedative to the description of case in which it may be deemed admissible, and a judicious admixture of various kinds of sedatives.* I do not think we pay sufficient attention to this fact. I have often seen an apparently incurable and unmanageable case yield to a combination of sedatives, which had resisted the operation of any one or two when given separately." (p. 75.)

Dr. Winslow specifies the sedatives, and the combinations with other remedial agents, which he has found useful in certain forms of insanity.

We pass over numerous practical remarks, to note specially *endermic medication* in insanity—a plan offering numerous advantages, but little practised. Dr. Winslow states on this point:

"In some chronic forms of insanity, in dementia, and persistent monomania, connected, as it was supposed, with morbid thickening of the dura mater, and with interstitial infiltration of the membrane, as well as with exudations upon its surface, I have occasionally had the head shaved, and have perseveringly rubbed over the scalp a strong ointment of the iodide of potassium combined with strychnine. In other instances I have kept the head painted with the mixture of iodine. I have seen marked benefit from this mode of treatment. When the mental symptoms are supposed to be associated with effusions of serum, I have ordered the iodine to be applied externally, at the same time exhibiting minute doses of calomel, or mercury-with-chalk, to slightly affect the system: this, conjoined with occasional tonics, diuretics, and stimuli to support the vital powers, is occasionally productive of considerable benefit, in cases apparently placed quite beyond the reach of improvement or cure." (p. 78.)

We have seen a *solution* of iodide of potassium constantly applied to the shaven scalp followed by improvement in the mental state. A cap lined with gutta percha muslin suffices to keep the scalp constantly moist. Cases of *dementia*, the consequence of *strumous* irritation or inflammatory action, are those in which endermic medication will yield the most satisfactory results.

Dr. Winslow concludes this lecture with some important remarks on

* A full account of this method may be found in the third number of the Journal of Psychological Medicine.

the serious consequences which are resulting from the idea, that insanity *is not amenable to medical treatment.*

"We see it," he observes, "influencing the conduct of county magistrates in the architectural proportions, medical organization, and general arrangements of our great national asylums. We also perceive the consequences of the error operating in many of the private institutions for the treatment of the insane, thereby degrading them into places of detention, instead of conferring upon them the character of HOSPITALS FOR THE CURE OF THE INSANE." (pp. 80, 81.)

These remarks are apposite. It is very true, and a subject of very great congratulation, that a manifest improvement in the treatment of the insane has been effected of late years. It cannot be denied, however, that there is a well-grounded fear that in the large asylums the curative treatment may degenerate into a simple hygienic plan. They are so under-officered, that the medical attendant cannot, from sheer impossibility (having only twenty-four hours in the day). give that minute attention to each individual case, which is often most essential to successful treatment. Then, again, it is necessary to the satisfactory management of these large establishments, that a *system* of government be strictly carried out; the inevitable tendency of this is to degenerate into routine, and routine practice (as is well known) is an insuperable bar to progress. If we add to these considerations another of not less importance—namely, the temptation to meet the wishes of economical magistrates and rate-payers, by making the *labour* of the inmates productive, we have sufficient grounds for expressing the opinion, that it is a great and good service to recal the practitioner's attention to the fundamental fact, that insanity is a disease of a viscus, and, like other diseases of the viscera, is amenable to medicinal agents.

The third lecture, "on Medico-Legal Evidence in cases of Insanity," comprises a practical statement of the cases in which medical evidence is required in courts of law; an examination of the legal *dicta* of the judges delegated by Parliament in 1843 to decide upon some fixed propositions as to the conditions under which a person is legally irresponsible, both theoretically and in relation to cases on record; and an analysis of the various terms used in courts. Dr. Winslow also gives practical instructions to the medical witness as to his conduct when giving evidence, and as to the manœuvres of counsel, illustrations being drawn from his own experience. All these are of importance to be known; this lecture should therefore be carefully perused by every practitioner who is about to occupy, before a commission of lunacy or elsewhere, the position of a witness.

The "safe rule" referred to in the subjoined paragraph is, we think, correctly characterized as the *only* safe rule; it is certainly the conclusion at which we arrived long ago, after a careful consideration of the question.

"Having, I think, conclusively established that we have no uniform legal or medical test of insanity to which we can safely appeal in criminal cases, you will ask, have I any psychological *criteria* to suggest for the safe guidance of the profession?—can I propound any principles which will assist the medico-legal witness in arriving at a satisfactory result? In reply to these interrogatories, I allow that we have no infallible standard, no certain principles which would admit of general and indiscriminate application. The only safe rule upon which we can act, is that of comparing the mind of the alleged lunatic, at the period of his suspected

insanity, with its prior natural and healthy manifestations; *to consider the intellect in relation to itself,* and to no artificial *à priori* test." (pp. 153, 154.)

Dr. Winslow quotes Dr. Combe's opinion to the same effect. We would suggest a further step in the inquiry—namely, to investigate how far the abnormal mental phenomena depend upon *cerebral disease;* for it is this which constitutes insanity; the morbid mental phenomena being only *symptoms.* This process will separate those cases in which the irregular mental action is *congenital,* but *normal* as to the individual, from those in which it is *induced* and *abnormal* as to the individual. In the former case, we have to investigate idiotcy, imbecility, or eccentricity; and these may be considered as removed from the domain of technical inquiry, to that of mere common sense, in which witnesses, counsel, judge, and jury are all equal.

Dr. Winslow finally calls attention to the important relations between the criminal and the insane mind, and, in illustration, quotes a remarkable table from No. 163 of the 'Quarterly Review.' It was not drawn up for the use to which Dr. Winslow applies it—namely, to establish "the painful fact, that there is in existence a large amount of crime connected by hereditary predisposition and descent with diseased mind." These unfortunately constituted persons occupy, as to their mental state, a sort of neutral position between soundness and unsoundness of mind; not legally or medically *insane,* but yet weak in self-control from an over-mastering organization. To these, as well as to the legally insane, Dr. Winslow argues, some mercy should be shown, and in this opinion, all who are practically acquainted with the subject, will fully and approvingly concur. So important a step in legislation, however, can only be taken when public opinion is more enlightened, and when the profession itself is so much better informed in cerebral physiology as to gain the confidence of, and so guide, the bench and the bar.

T. Laycock.

REVIEW III.

Traité des Maladies du Sein et de la Région Mammaire. Par A. VELPEAU, Membre de l'Institut, &c.—*Paris,* 1854. pp. 717.

A Treatise of the Diseases of the Breast and of the Mammary Region. By A. VELPEAU, &c.

WE are informed by the renowned Professor of La Charité, that this voluminous monograph is composed of materials collected during a public and private practice of thirty years. Nearly two thousand cases constitute its basis. From an experience of this magnitude we might be led to expect a considerable amount of novelty; and how these expectations are realized it will be the object of the following analysis of the work before us to exhibit.

The author disposes of the anatomical and physiological conditions of the mammary gland in less than twenty-one lines.

He arranges the affections of the mammary gland of the female in two principal categories.

"1. The diseases of benign nature, inflammatory and not inflammatory.

"2. The diseases of a malignant nature or cancerous."

Those of the first class are thus defined:

"By benign diseases I mean those which, left to themselves, do not fatally menace life, or of which cure is the natural termination. I divide this class into two groups, one of the different kinds of inflammation, the other of those diseases which, from the first, or in their course, are foreign to inflammatory action." (p. 2.)

The first chapter is devoted to the subject of the inflammatory diseases of the breast, and comprehends excoriations, fissures, eczema, various forms of erysipelas, congestion of the gland tissue with milk, and all the varieties of phlegmon. Of these various diseases, some attack the nipple or the areola, whilst others are confined to the lactiferous ducts. Many originate either in the gland alone, or in the cellulo-adipose tissue.

The first section of this chapter is devoted to the eczematous affections of the nipple and areola, to excoriations and fissures or chaps of the nipple, and to inflammation and abscess of these important parts of the organ.

The second section treats of inflammation of the breast, properly so called, of which the subjoined table represents certain divisions.

"Phlegmon	1. Inflammation, superficial or sub-cutaneous,	circumscribed. diffused. primitive. secondary. complex.
	2. Inflammation, deep or sub-mammary,	idiopathic. symptomatic. general or diffused. partial or circumscribed. primitive or secondary.
	Congestion from Milk. 3. Inflammation, glandular or parenchymatous,	of the milk ducts. of the gland tissue. complex." (p. 26.)

Lactic congestion of the breast is illustrated by a table of 25 cases, and M. Velpeau thus sums up the section:

"Cases of lactic congestion are so numerous, the progress and the symptoms of the disease exhibit so much simplicity, that it would be quite superfluous to accumulate in this chapter a larger number of examples.

"1. In 25 cases, the disease affected—

Both breasts	in 7	individuals.
The right	in 9	"
The left	in 9	"

"2. Of the patients, 24 had been only just confined, or were suckling, and only one was pregnant.

"3. The congestion was attributed to—

Weaning in . . .	7	individuals.
Cold in	7	"
Contusion in . .	5	"
Parturition alone in	6	"

"4. The respective ages of the individuals were as follows:

Aged 19	years,	2
20	,,	3
20 to 30	,,	12
30 to 40	,,	5
40 to 50	,,	2
55	,,	1

"5. A complete cure was the result in—

From 4 to 15	days in	10	individuals.
,, 15 to 30	,,	3	,,
,, 30 to 40	,,	3	,,
,, 40 to 60	,,	2	,,
,, 60 to 80	,,	2	,,

and five women left the hospital before the cure was effected.

"6. Finally, in 18 cases the congestion was purely of milk, whilst in 7 others it was either inflammatory, chronic, or foreign to lactation." (pp. 74, 75.)

It appears remarkable that M. Velpeau does not allude in the third group to the defective development of the nipple as a prolific cause of lactic congestion. In our experience, this condition of that important portion of the organ is by far the most frequent source of trouble.

In the following *résumé*, M. Velpeau gives the result of his experience:

"From the preceding observations, it follows that inflammation may begin in one or other of the elements constituting the mammary region. 1st. In the skin, under the form of erysipelas or eczema. 2nd. In the subcutaneous tissues, under the form of phlegmon or absorbent inflammation. 3rd. In the submammary tissues. 4th. In the gland tissue itself, under various forms. 5th. In a form more general still, in the gland tissue, or in the connective tissue. . . . .

"To justify these distinctions it is only necessary to attend to the special characters which the inflammation presents, either in its causes, its symptoms, its progress, its prognosis, or in the treatment which is applicable to it. Thus, subcutaneous idiopathic inflammation arises in the breast under the influence of the same causes as in all other regions. Deep inflammation may result from external violence, or from certain affections of the chest and axilla, but it does not the less frequently arise from diseases of the breast itself. As for phlegmasia of the gland, properly so called, there is a large amount of evidence to prove that it almost exclusively depends upon lactation, parturition, or pregnancy.

"As regards *symptoms*, who has not observed that an inflammation characterized by redness, a circumscribed or diffused swelling, elevating the surface of the skin, and soon accompanied by a sort of œdema, differs essentially from that which, situated under the breast, shows itself from the first by raising and pushing forwards the entire gland: and continues, even to the end, without causing any marked redness or very distinct lumps on the surface of the region? And then, how can these inflammations be confused with those, such as of the gland tissue, which manifests itself, from the first, under the form of lumps, more or less deep and numerous: which, preceded or complicated by the suppression or retention of milk, frequently involves several regions of the breast at one time?

"Subcutaneous inflammation, like common phlegmon, is scarcely more than eight days in terminating in an abscess, which is generally single, and the fluctuation of which, as soon as fluctuation is established, rarely escapes the attention of the surgeon. Deep phlegmon, on the contrary, even when developed more quickly, exhibits this which is remarkable, that when the suppuration is established, it is recognisable at a much later period. From being deeply seated, abscesses, when they form, have not the advantage of opening so quickly as those the result of subcutaneous phlegmon; but the pus traverses the gland from behind forwards,

and then gives rise, secondarily, to subcutaneous phlegmon. Parenchymatous phlegmon differs from the two preceding, and almost always depends upon several phlegmonous attacks, which continue to arise with some women for one, two, or three months consecutively. The same well-marked distinctions characterize the termination and prognosis of these cases. Setting in with violence, from the first, superficial and deep phlegmon sometimes terminates in resolution. In the gland itself, on the contrary, inflammation almost inevitably proceeds to the formation of one or two abscesses.

"As regards therapeutical means, there is always a better chance of a successful issue if special means be employed to combat each form of inflammation. A great number of leeches on the diseased region, mercurial ointments, compression, and the usual topical applications succeed very well in subcutaneous phlegmon. They are quite useless, on the contrary, in deep inflammation, as well as in parenchymatous. Deep phlegmon is aggravated by compression, and requires general bleeding or leeches around the breast, and large cataplasms, as well as mercurial ointments. It is to inflammation of the gland tissue that purgatives, *les tisanes altérantes*, and topical applications, purely emollient, are most advantageous. Ammoniacal, camphorated, and anæsthetic liniments are applicable only to engorgements from milk.

"Lastly, when we see that, in spite of every kind of treatment, mammary adenitis is often prolonged by repeated attacks, a great number of weeks, whilst by appropriate methods subcutaneous phlegmon, as well as submammary, rarely lasts more than eight or fifteen days, the importance of the distinctions before established ceases to be a subject of debate." (pp. 84—7.)

The third section is devoted to the consideration of abscess of the breast, which the author thus divides:

"1. Subcutaneous or superficial abscess; (*a*) of the areola; (*b*) of the celluloadipose tissue; primary, secondary.
"2. Deep abscess; (*a*) idiopathic; (*b*) symptomatic.
"3. Glandular or parenchymatous.
"4. Chronic abscess." (pp. 87 and 152.)

A table is appended of more than 200 cases, of which the following analysis is given:

Patients cured	139
Deaths	3
Incomplete cures	28
Left hospital not cured	5

"Twenty-one cases exhibited complications, arising from—

Erysipelas in	5
Abscess in axilla in	6
Pleurisy in	2
Abscess in neck and back in	2
Variola in	2
Phthisis, eczema, erythema, and gastric disturbance in	4

"As regards the gland affected:

The right breast in	75
The left breast in	75
Both breasts in	23

"The age of the patients:—

From 15 to 20 years	30 cases.
From 20 to 30 years	116 ,,
From 30 to 40 years	23 ,,
From 40 to 50 years	5 ,,
From 50 to 60 years	3 ,,

"*As regards the site of the abscess it was:*

Subcutaneous in	37 cases.	
Submammary in	38	,,
Parenchymatous in	95	,,

"The causes to which the disease was attributed:

Contusion in	20 cases.	
Eczema in	3	,,
Pregnancy in	7	,,
After parturition in	110	,,
Fissures, only	2	,,
A needle	1	,,
Cold	6	,,

"In relation to lactation:

In women who have suckled	75
,, ,, ,, not suckled	4." (p. 197.)

In the second chapter, the diseases of a benign nature, and which are not dependent upon inflammation, are described; the author first discusses "Ecchymosis without contusion, or spontaneous," and "Contusion properly so called." Then, in the second section of this chapter, he refers to "indolent tumours of a benign nature."

A table of these tumours, proposed by the author in 1838, is inserted; it comprehends tumefactions; chronic intumescence; all tumours which, left to themselves, are not exposed to cancerous degeneration, or which are very rarely subject to this unfortunate transformation; hypertrophy; engorgements; a certain number of cysts; and many varieties of tumours. It is as follows:

"1. Hypertrophy . . . .
- *a.* Of the gland.
- *b.* Of the connective tissue.
- *c.* Of the adipose tissue.

"2. Engorgements . . . .
- *a.* Of the connective tissue.
- *b.* Of the gland tissue.

"3. Cysts . . . .
- *a.* Sebaceous.
- *b.* Gelatinous.
- *c.* Hæmatic.
- *d.* Lactic.

"4. Tumours . . . . . .
- *a.* Lipomatous.
- *b.* Fibrinous.
- *c.* Butyrous.
- *d.* Tuberculous.
- *e.* Osseous.
- *f.* Granular or nodulated.
- *g.* Adenoid, called partial hypertrophy.

"5. Imaginary or supposed tumours." (p. 209.)

In a section headed "Indolent Engorgements," M. Velpeau very properly objects to the term in these words:

"It is too vague a denomination, and of too little value, to be retained in precise scientific language. Before pathological anatomy permitted the appreciation and analysis of the changes and alterations induced by disease in the tissues, it was necessary to make use of a word which at least indicated the most evident charac

teristic; that is to say, increase in the volume of the part diseased. In the present day, however, the various lesions of which the breast is the seat are sufficiently well appreciated to examine them singly, to isolate them, and to give the history of each individual kind. It is, then, only in a collective sense that we can apply the term 'engorgement.' Otherwise understood, in regard to the mamma, it denotes just as much poverty in scientific nomenclature, as that of 'white swelling' applied to the pathology of the joints." (p. 210.)

Under the term "Physiological Engorgement," that affection of the breast is described which is so commonly met with in young single women prior to the catamenial period, in the early months of utero-gestation, or as an accompaniment of some irregularity or depravity of the menstrual function.

The author uses the term physiological, because this affection is in such close relationship with the procreative system, and because it does not strictly deserve the name of disease.

"Simple Engorgement," partial or diffuse, results from contusion and different sorts of irritation,—as the influence of pregnancy, lactation, or depravity of the catamenia.

"Hypostatic Engorgement" expresses a condition of the gland which appears to originate in its own weight, or from its dependent position. There are two forms:—1st. That which occurs in women who have had children, and whose breasts are soft, heavy, and pendulous, and the pathological condition of which when swelled can be only attributed to stasis of the fluids and the pendent state of the organ. 2nd. In women whose breasts are large, without being soft or pendulous, but which are very fat, and in whom engorgement of the axillary region of the organ is often observed. The last form the author attributes to the pressure of badly fitting stays, which press the parts against the axillary region.

The consideration of "Symptomatic or Consecutive Engorgement," and of "Engorgements with a Tumour," concludes this section, in which the author does not appear to us to have kept in view the observations with which it commenced. A perusal of the cases appended to this section will, we think, bear us out in this statement. (p. 230.)

"Tumours, properly so called," form the subject of the next section of this second chapter. M. Velpeau writes:

"The benign tumours of the mamma may be referred to two principal divisions: those which are composed of solid substance or tissues, and others of fluid or pultaceous matter. There are, in other words, solid tumours or cysts. This last term must not be understood to the letter, however, because certain solid tumours are quite as well surrounded by a sack as the collections of fluid which the cysts specially characterize.

"Solid tumours are subdivided into several kinds: some composed of the natural elements of the economy, more or less modified, exhibit a kind of texture resembling certain tissues; in others, on the contrary, new products are formed, a proof, at least, that the disease has originated in an exudation of matter effused from the vascular system, under the influence of abnormal actions.

"Hypertrophy, lipoma, and neuromata, belong to the natural elements of the breast, and merit, therefore, a serious study." (pp. 230, 231.)

"Hypertrophic Tumours." Under the term hypertrophy, M. Velpeau only describes those intumescences devoid of notable change in the contexture of the breast:

"*In hypertrophy the organ offers a manifest excess of volume; its interlobular spaces (mailles), its lamellæ, its cellules, its lobules, in fact all its elements, have* undergone an abnormal development, which may be extreme, but without being complicated with disorganization; in such a manner that, when placed by the side of a section of the healthy tissues of the breast, the tumour cannot, at first sight, be distinguished from it. It is, then, in their proportions, not in their constitution, that the elements of a hypertrophied breast have undergone change." (p. 231.)

M. Velpeau insists the more on this characteristic, because he has to return to the subject when describing the adenoid tumours which many modern pathologists describe as hypertrophy of some of the lobes of the mamma, which appears to him very *mal à propos*.

"Hypertrophy exhibits two varieties: diffuse or general, and circumscribed or partial." (p. 231.)

M. Velpeau next describes ordinary cases of hypertrophy of the breasts, without adding to our knowledge of either the pathology or treatment of this fortunately rare disease. He then concludes the subject by the relation of a case which, so far as reported, demonstrates sufficiently clearly that it was not an example of hypertrophy of the mamma at all, but an entirely new growth which he removed. M. Velpeau gives an illustration of a section of this morbid structure, Plate I., which certainly in nowise resembles a section of a healthy mammary gland.

In the case referred to the disease was developed at the age of forty-two, in a prolific, healthy woman. "The *tumour*, formed by the entire breast, and not at the expense of a part of it only, exhibited irregularities of very different consistence." The dissection of the mass removed is thus described:

"The skin was thin, without adhesion, and the subcutaneous fat had completely disappeared from between the tumour and the skin. When the lobes were cut they projected upon the surface of the section. The mass of the tumour was not formed of a homogeneous tissue, but it was divided into several lobes, perfectly separable, and between which a very loose connective tissue was seen. Each of these lobes was composed of two elements: one projecting on the surface, elastic, grey, granular, and forming little nodules of various sizes; the other having a pearly lustre (*des nuances franchement irisées*), but in reflected light only, and of which the fundamental tint was white. The firm tissue, being very elastic, seemed to retract between the greyish-red nodules. The proportion of these two tissues was not the same in all parts of the growth. At the superficial part of the breast the very small lobules seemed to be merely surrounded by an embroidery of white tissue, whilst in the deep part, large and more scattered, they were surrounded by real bands, as it were. At the deeply-seated part of the tumour there was a bursa mucosa distended with serosity.

"The microscope, as well as an examination with the unassisted eye, demonstrated that all the lobules belonged to hypertrophy, and that they all contained the glandular cul-de-sacs." (p. 239.)

We have given the relation of the dissection of this morbid growth *in extenso*, because it demonstrates that, even in the present state of knowledge, how much confusion and difficulty exists in the appreciation of the new growths met with in the breast; and also to show how very deficient in details are several of the cases recorded by M. Velpeau. In this instance, for example, no mention whatever is made of the mammary gland itself—neither whether it was entirely removed with the

growth, nor, if removed, what were its relations to the growth. If the gland had been entirely atrophied, the ducts should have been traced, and then, in all probability, as we have frequently seen, the growth would have been found to be developed behind the atrophied organ. The confusion in the appreciation of the new growth is demonstrated by placing this morbid product in the category of cases of hypertrophy; for M. Velpeau, by so doing, here contradicts himself, after having just before stated that, in hypertrophy, the growth is merely of breast gland-tissue, and differs not, in any way, from the appearance of a section of the normal gland: whilst, in the tumour he describes, the appearance of the section was as far removed from that of a breast as well could be. In fact, this new growth, and "tumour," as M. Velpeau very properly styles it, was really a tumour, and not hypertrophy. It was, doubtless, one of the varieties of mammary glandular tumour, or adenocele.

Observations similar to the above apply to the subject of "partial hypertrophy," thus described by the author, and in which category he includes "fibro-cystic tumours" and "cysts."

With reference to "Lipoma," M. Velpeau observes:

"It would be wrong to suppose that adipose hypertrophy, before spoken of, can be confused with lipoma—that lipoma is only a variety, a shade of adipose hypertrophy of the breast. The lipoma which I have observed in the mamma exhibited exactly the same characters as lipoma in every other region of the body: they were composed of lobulated masses of fat, of isolated adipose-cells, and were quite distinct from the surrounding tissues. In the persons thus affected, the rest of the natural adipose layer did not exhibit the least appearance of hypertrophy." (p. 246.)

Two cases are related in which tumours of this class were removed during life.

"Tumours dependent upon an Alteration of the Mammary Tissue" are next considered; and the author divides them into "Simple Mammary Induration," and "Neuromatic Tumours and Nodosities." The first heading is subdivided into "Induration with Sub-acute Swelling," and "Chronic Induration." (p. 253.)

The species of mammary induration of which M. Velpeau here writes, differs from the engorgements which he before described

"In the fact, that all the tissues which surround the diseased part of the gland retain their suppleness and the other characters which belong to their normal state. (p. 253.) . . . .

"Chronic induration," described by M. Velpeau, in 1838, under the title, 'Induration *en Masse*,' "is a disease which was until then confused with tumours of every other kind. This disease, in the diagnosis of which it is, perhaps, difficult to be skilled, even at the bedside, is characterized, like the preceding—which is sometimes its point of departure, or first period—by manifest induration of a part or the whole of the gland. In general, it can only be appreciated and, in reality, determined by a comparison of the healthy breast with that diseased, and by the inequalities in density of various lobes of the gland. Sometimes accompanied with lancinating, heavy, and dull pains, it comes on by slow and insensible degrees; the gland then appears lumpy, having sensibly increased in volume; nothing indicates that it is the seat of the least engorgement or inflammation. It is only by pressing in succession the different lobes of the breast directly against the chest, or by grasping the lobes from side to side, and drawing them from the thorax, that it can be determined that some of them are manifestly more hard and unequal than the others. . . . . This affection, which it is quite possible to confound, at first, with scirrhous degeneration, deserves all the attention of surgeons. Left

to itself, it often disperses without leaving any trace of its existence. It has never been demonstrated to terminate in the establishment of a scirrhous or encephaloid tumour." (p. 255-56.)

After describing the usual methods of treatment in these cases, M. Velpeau asks—

"Is it sometimes necessary, or even useful, to extirpate these tumours? It has been shown, when describing their prognosis, that, to all appearance, the benign induration of the mamma has no tendency to degenerate into a malignant tumour. In my opinion, then, an operation with the knife is not indispensable in such cases. It becomes, therefore, a matter of the greatest practical importance not to confuse this kind of induration with scirrhus: the one is nearly always cured without operation, and only very rarely gives rise to serious consequences; whilst the other can only be treated effectively with either cutting instruments or escharotics. In the one case, we may always completely reassure the friends of the patient; in the other, the most serious prognosis can only be pronounced." (p. 257.)

At a subsequent page, Velpeau, under the title of "Neuralgic Pains," refers to the cases, so common in the early and middle periods of life, in which there are severe pains, and sometimes feelings of heat, and yet in which no appreciable change in the tissues of the affected organ is manifest to touch or sight. The affection seems to depend upon a state of hyperæsthesia, and is connected with more or less general derangement of the functions of the procreative organs. The author adds nothing to our present knowledge as regards either the pathology or treatment of this complaint.

Under the heading of "Pain and Imaginary Tumours," the author describes those cases which arise after abscess, contusions, compression, and in irritable, excitable, nervous females, in whom the catamenial function is imperfectly performed, or is associated with leucorrhœa, and a generally disordered state of the nervous system. Several interesting cases are related, but the morbid anatomy of this state of the gland-tissue is not described. It is well known, and has been demonstrated by the use of the microscope, that these lobes of gland-tissue, thus affected, exhibit a very peculiar condition.

The cæcal terminations of the ducts which, in the inactive state of the organ, are usually void of epithelium, or only contain a delicate layer of it, are completely filled with this characteristic structure; and they appear like those of a gland in preparation for the secretion of milk. This fact is not mentioned by M. Velpeau.

"Tumours formed of Excreted (*Exhalées*) or Effused Materials."—Under this title we have described lymphatic or tuberculous tumour; disseminated, multiple, and purulent. Some of these cases appear to be what we should term chronic inflammation, effusion, or abscess; for there is not sufficient evidence in their relation to entitle them to rank as a distinct class of disease.

"I will add," writes M. Velpeau, "that the tumours which seem most peculiarly to merit the title of tuberculous, often finish by dispersing, by becoming inflamed, or by transformation into an abscess, which commonly brings along with it the cure. . . . . And lastly, we must admit, that certain tumours of the breast, termed scrofulous, or tuberculous, result rather in a form of chronic inflammation, in lymphatic females, than in any peculiar morbific act." (p. 293.)

"Osteoid; Osseous or Calcareous Tumours," are then described. Old cases only are quoted.

"Tumours formed of Milk, or Galactocele," are divided into those by infiltration, those circumscribed by a cyst, and those which are solid or concrete.

"*In a sac, formed at the expense of the neighbouring tissues, or in a cavity resulting from the dilatation of its proper canals, the effused and accumulated* milk would still undergo certain changes, perhaps like blood, when placed under similar circumstances.

"Thus: 1st. It would continue in the state of a slightly painful collection, without notable change, as was the case in a patient under Scarpa, for example, or that of M. Forget.

"2nd. It might decompose, and be replaced by a liquid purely serous, or by a mixture of serum and caseum, as in the case related by Sir A. Cooper; or else, if the serous part is taken up, it may become thick and creamy, like one of the cases of Dupuytren.

"3rd. It may inflame and transform the tumour into a true milk abscess, which, after being a long time indolent, assumes from this moment the progress and characters of an acute attack.

"4th. It may form concretions susceptible of assuming any kind of form and appearance, even to give rise to the idea of milkstones.

"5th. Becoming harder and harder, like the fibrine of blood in hæmatocele, and giving rise to butyrous and caseous tumours, which were seen in some of M. Velpeau's own cases." (pp. 307, 308.)

The differential diagnosis of these tumours formed by milk is next considered, and the author admits that the difficulty in distinguishing between them is great, chiefly owing to the very slight shades of difference which they exhibit.

The subject of the next section is "Cysts in the Mammary Region," and the following varieties are described:—(1) sebaceous cysts; (2) hydatids; (3) serous cysts; (4) sero-sanguineous cysts; (5) sero-mucous cysts.

Of the first variety, a very good illustration is given; but the case exhibits no anatomical difference from the ordinary sebaceous tumour of other parts of the body.

M. Velpeau writes of the second variety:

"Let us first state, that it is not demonstrated that all the tumours described under the title of hydatid were really so constituted. On reading the cases described by authors, serious doubt of the fact arises." (p. 316.)

The present state of knowledge upon this subject fully confirms the suspicions of M. Velpeau, for it must be admitted that many cases described under the title of "hydatid," really belong to the class of serocystic disease of the breast.

The true, genuine hydatid cysts, characterized by the presence of echinococcus hominis, are developed certainly in the mamma. This fact is demonstrated by recent observation.

The remarks upon the third variety present no novelty requiring a detailed notice; and the fourth and fifth varieties are named in accordance with their contents. The author has not enunciated any new pathological views, nor does he introduce any novel plan of treatment in these cases. In point of fact, his anatomical details are rather behind our present amount of knowledge, and this deficiency, which occurs throughout the volume, must be attributed to the want of careful and minute investigation by means of the scalpel, and by the aid of the microscope.

"Adenoid Tumours" are then described. These, says the author, have

"Generally, in surgical treatises and practice, been confounded with scirrhous

and encephaloid tumours, with occult cancer, and with benign scirrhus, and their history has not yet been sufficiently studied." (p. 350.)

Under the name *Tumeurs Fibrineuses*, M. Velpeau described these tumours in the '*Dict. de Méd.*,' tome xix., and in his lectures, more generally, under the names of fibrous, scirrhous, or adenoid.

M. Velpeau seems to take great credit to himself for observing these tumours, and, at the same time, throws in the background the observations of others. The following description was given by M. Velpeau of one of these tumours, in 1824.

"It appeared," he then said, "that it was a degenerescence or transformation of the cellular tissue, and not a new formation. In a word, that it was a fibro-cellular nucleus, indurated by the morbid action; in fact, if not deceived in the utility of distinctions attempted to be established elsewhere, when speaking of carcinomatous tumours, a patient, the subject of this disease, might reckon on a complete cure." (p. 350.)

Astley Cooper, as Velpeau remarks, calls them "chronic mammary tumours;" his observations were published in 1829, under the title, 'Illustrations of the Diseases of the Breast.' Many years, however, before this period, he had described these tumours in his lectures on surgery, both anatomically and physiologically, and especially as regards their innocence and resemblance to the structure of the organ in which they were developed. M. Lebert states the fact clearly and honestly, when he writes in his most valuable work on 'Cancerous Diseases, and the Curable Affections confounded with Cancer'—

"The author who has best described these tumours is Astley Cooper; but yet it is clear, after reading his work, that he has not taken for his description of these cases, all the types under which these tumours show themselves, and are developed." (p. 367.)

M. Velpeau states nothing new as regards either the natural history, diagnosis, prognosis, or treatment of these cases. His facts agree with the observations of others, and as regards the anatomy of these new formations, his details are, literally, behind our knowledge of them in this country. This deficiency is solely attributable to the want of personal examination with the aid of the microscope; but he does not fall into the error, as some of his countrymen have done, of regarding these tumours as hypertrophied mammary tissue. These tumours cannot be regarded as hypertrophy, in the strict acceptation of the term, because their structure is, at best, but an imperfect imitation of the mammary gland tissue.

"In my opinion," writes M. Velpeau, "the creation of tumours in general is naturally influenced, often modified, by the neighbouring organic media. I have said for a long time, and I repeat it, that accidental formations have a manifest tendency to assume some of the characters of the organ in which they are located." (p. 357.)

Now, this doctrine was first enunciated, we believe, by Mr. Lawrence in 1832, and may be read in the 'Medico-Chirurgical Transactions,' volume xvii.

M. Velpeau adduces, at p. 391-92, three cases in proof of the statement that the tumours termed by him "adenoid" can be absorbed or resolved by topical applications.

The following are the opinions of M. Velpeau regarding the treatment of these tumours:

"Although I profess that these tumours remain benign even to the end, that they are not susceptible of undergoing a malignant or cancerous transformation, I nevertheless think that it is better to extirpate them than to abandon them to themselves, or treat them by medical means and simple topical applications.

"This is the rule I have adopted, and for a long time recommended, in similar cases.

"1. If the patient is resigned to her condition, little disposed to trouble herself about a tumour, already of long standing and stationary, I advise her not to think about it, to leave it alone, and only to examine the lump at long intervals of time.

"2. To those who are naturally excitable I attempt to explain that the tumour of itself can give rise to no danger, and that it is not susceptible of assuming a bad character; I advise them, moreover, as much to calm their imagination as in the hope to cure them, to employ one or other of the medicines above described.

"3. I insist on the employment of these medicines when consulted by timorous females, who dread excessively the thought of an operation, immediate or remote.

"4. When the patient is very much occupied by thinking of the tumour, and the dangers which she attributes to it alarm her more than the operation itself, I then advise her to submit to its removal.

"5. Lastly, I tell other patients that there is no danger attending the existence of the tumour, but that if they wish to be cured of it medical means offer little chance of success, besides requiring a considerable amount of time, whilst the operation certainly removes it without exposing them to serious danger; and I add that, once extirpated, this species of tumour is not reproduced." (pp. 401, 2.)

This chapter is profusely illustrated by cases, and at the end a table of 60 is given. Upon looking over them a certain amount of suspicion is felt at the advanced age of many of the patients, but it must be observed that the age of the individual at the time she fell under the notice of M. Velpeau is there recorded, and in another column the length of time the tumour had been growing. Thus we find that in 1853 a tumour of this nature was removed from a woman of 60 years of age, but it had existed 20 years, and was, therefore, primarily developed at the age of 40.

The age at which these tumours are developed is very important as regards their diagnostication, and, therefore, this fact should be always accurately ascertained. This table concludes the first section and more than the half of the whole work.

The second section of the work is devoted to the consideration of diseases of a malignant nature, or of cancers of the mammary region.

M. Velpeau writes: "Cancer of the breast differs neither in its nature nor in its form from cancer of other parts of the body;" and he proceeds in the first chapter to denote the various forms of cancer, to which he applies the following terms:

"Scirrhus:
- I. Ligneous scirrhus.
  - *a.* Scirrhus, properly so called, or globular.
  - *b.* Rayed scirrhus, or branching.
  - *c.* Scirrhus "*en cuirasse*," or tegumentary.
  - *d.* Ligneous in mass.
  - *e.* Atrophic scirrhus.
  - *f.* Pustular, or disseminated.
  - *g.* Scirrhus of the lactiferous ducts.

II. Lardaceous scirrhus.
Partial and diffuse.
Encephaloid.
Melanosis.
Chondroid, colloid, and fibro-plastic cancer.
I. Napiform tumours, or fibro-plastic, properly so called.
II. Colloid cancer.
Epithelial cancer, or epithelioma.
Keloid.
Anomalous cancer."

The second chapter of this second section is devoted to the consideration of the differential diagnosis of the varieties of cancer, by their microscopical anatomy, and between cancer and benign tumours.

The third chapter treats of the nature and etiology of cancer, and the origin of this disease as dependent upon external violence, is considered. M. Velpeau thus writes upon this important point:

"Without absolutely denying the influence of external causes in the production of cancers, some authors restrict themselves by saying that a special predisposition is at least required; that without such predisposition, the external cause would not have been followed by such a result.

"It seems, in fact, since we are unable to generate it when we wish, that individuals affected with cancer have in their organism a certain predisposition to contract it; but this predisposition being admitted as a fact, science is scarcely more advanced. All diseases might invoke the same peculiarity. Without the predisposition, phthisis would be only rarely established. The affection called scrofulous, does it not also require an organic predisposition? Are there not certain individuals predisposed to lipoma, tumours, and steatoma? Does not pneumonia itself require, in the greater number of persons it attacks, a special predisposition? Arising from the slightest causes, whilst a similar cause, much more severe, produces nothing of a like nature in an infinity of other persons, it is natural to admit for cancer a special predisposition; but that does not in any way prevent the necessity of an occasional cause, without which it would not manifest itself." p. (532.)

The predisposing causes of cancer are next related; these are age, sex, general health, constitution, regimen, habits of life, &c.; and the consideration of the occasional causes and contagion concludes this chapter.

The prognosis of cancer is described by our author as hopelessly unfavourable.

The remedies employed to combat cancer of the breast, are internal medicines and external applications. The first are bleeding from the arm, or by leeches from the part; purgatives, emetics, preparations of conium, arsenic, alkaline substances, preparations of gold, quinine, sarsaparilla, and iodine. The use of these remedies is not recommended by the author, they are rather mentioned as having been employed, and their inutility demonstrated; and M. Velpeau thus concludes:

"To sum up in a few words, the cancerous nature of the disease being once established, we do not yet possess any remedy, any general or internal medicine which has ever brought about a cure. Besides my own experience, I have, in support of the opinion to which I have just given utterance, the examination of many facts occurring in the practice of others. Every time that I have wished to verify the observation invoked in favour of such a mineral water, or such pretended curative treatment, I have arrived at the conviction that the nature of the tumour was mistaken, or that the pretended cure was not realized." (p. 561.)

The second, external applications, are the same as we employ in this country. In relation to the efficacy of the employment of compression in cases of carcinoma of the breast, M. Velpeau writes:

"Thus I say to practitioners without hesitation, that they can place no reliance on the efficacy of this resource in the treatment of cancers. If it sometimes succeeds, it is only, be assured of it, in cases of simple engorgement, or of tumours not cancerous." (p. 563.)

As exceptional cases, M. Velpeau relates at pages 565 and 6 three very interesting observations, "not with the sole view of proving that scirrhus (*les squirrhes*) may be cured; but with the hope of inducing practitioners not to deny absolutely the possibility of the fact at the commencement of the disease, and under the forms he has indicated."

M. Velpeau concludes a chapter on the appreciation of the curative means in these words:—"If the curative insufficience of so many different medicines is but too plainly demonstrated, must not the same be said of operative medicine, of surgery properly so called?"

The surgical means to be employed, and especially the question whether cancer itself can be radically cured by operation, are the subjects next discussed; and from clinical facts and observations M. Velpeau concludes:

"1. That no plausible reason has yet been given in favour of cancer being a disease primitively general.

"2. That, on the contrary, one would be inclined to admit its title to being an affection primitively local.

"3. That certain tumours of a benign nature *appear* to undergo, in some cases, a malignant transformation.

"4. That the cause of benign or malignant tumours, adenoid and even cancerous, of the breast may *probably* be traced to a plastic, sanguineous, or secreted exudation into the tissues, either spontaneously, or from external violence.

"5. That the existence, or the non-existence, of the cancer cellule in tumours is not conclusive evidence that the disease will or will not return after operation.

"6. That it would be, consequently, imprudent to decide from the evidence afforded by the microscope alone, whether to operate or not.

"7. That observations and statistics are far from proving that the extirpation of tumours of the breast is always followed by a return of the disease, always useless, or even hurtful.

"8. Finally, that facts sufficiently numerous, and that observations selected from my own practice, demonstrate, without the possibility of contradiction, the existence of radical cures of cancer by the operation." (p. 598.)

After describing the dangers of the operation by means of the knife, and the treatment of the case preparatory to its performance, the value of caustics in general, their advantages and inconveniences, form a subject for consideration.

Upon the practical value of the use of caustics M. Velpeau thus remarks:

"The result of my experience proves that they should not be rejected absolutely as a curative means. That they are preferable to a cutting instrument,—1st. When the cancer is ulcerated in patches, and when more widely spread than deep; 2nd. When, even by the cutting instrument, there is no chance of preserving a part of the integuments attacked by the tumour; 3rd. When the cancer is fungating, exactly limited, and when the patient dreads much more the use of the knife; 4th. Ulcerated scirrhus, irregular or disseminated, can be better attacked by caustics than by operation; 5th. The same may be said of ulcerated cancer adherent to the summit of the axilla, under the clavicle, or in the neighbourhood of bone." (p. 663.)

The effects produced by various kinds of caustics are then described, and their individual advantages examined.

Congelation, by means of the application of pounded ice and salt, has, according to M. Velpeau, certain advantages, which may be regarded as palliative, if not, even in some cases, as a means of cure. And, although the author has little experience in its application, the effects which he has seen produced would induce him to employ the frigorific mixture before entirely rejecting it, especially as a succedaneum to caustics.

"When a true cancer" has been operated upon, there always remains the sad expectation of a recurrence, and therefore M. Velpeau next takes into consideration the means to be adopted in the hope of arresting or preventing this distressing result.

After speaking of the employment of counter irritation, regimen, and syphilization, he thus very justly denounces the last:

"Syphilization, with the intention of curing or preventing cancer, is then one of the thousand chimeras which arise, from time to time, as an exhalation from the brain of certain men. . . . .

"In conclusion, science does not yet possess any means which may serve as a preservative from cancer, or prevent the recurrence of this terrible disease." (p. 682.)

The propriety of removing secondary developments forms the subject of the next section, and M. Velpeau is in favour of a second or even third operation, when practicable. He supports this doctrine by the relation of a case entitled "Encephaloid tumour, extirpated three times, and at last radically cured."

A lady, sixty-six years of age, had had a tumour removed from the breast a year before she consulted M. Velpeau. He removed the second development, which was in the lower edge of the cicatrix. The axillary glands were not diseased, nor was there a tumour elsewhere. The growth M. Velpeau removed exhibited "all the characters of encephaloid tissue. Soft, fungous, medullary, red, vascular on its external surface, it was lardaceous, homogeneous, brownish in places, and continuous everywhere with the thick layer of mammary tissue which was removed with it."

A third growth formed in eighteen months, was removed, and exactly resembled the last described. The lady had enjoyed perfectly good health since, and there is no new growth in 1853, ten years having elapsed since the last operation.

If we accept this case as one of encephaloid cancer, it does not prove more than that a patient may be free from the disease for ten years, even after the removal of a third development. It does not prove that she will continue free from cancer the rest of her life, nor that the disease is radically cured, because there are cases on record in which a cancer having been excised, and the patient having continued free from disease in the part primarily involved, has fallen a sacrifice to the development of internal cancer after a longer period than ten years had elapsed.

In the absence, however, of positive evidence, by minute examination of their structure, that these growths were cancer, we cannot accept this case in evidence merely because M. Velpeau states that, in his opinion, the growth was an "encephaloid tumour,"—which it might indeed have been without being a cancer, for many of the forms of mammary glandular

tumour are brain-like, but their minute anatomy at once demonstrates that they are not cancer.

A table of 146 cases concludes the subject of cancer. In an analysis of this table, M. Velpeau begins by stating that in a given year we find a certain proportion of cases reported. But the author's enumeration appears strangely inaccurate. Between the years 1824 and 1852, inclusive, M. Velpeau enumerates 190 cases; whereas the table includes only 146. Of 197 cases, 57 were not operated upon, and the operation, or cauterization, was performed in 140 cases. Of these 140, exactly half, 70, continued well, or were in progress of cure when they were lost sight of. Thus only 70 cases remain which can be of any use to determine the advantage or disadvantage of any operation in the treatment of cancer. Of these 70 cases, in 22 the disease had returned, and 30 patients were dead, leaving 18 concerning whom there is no more history.

Another equally unsatisfactory table of cases of encephaloid disease is given. In this, of 45 cases operated upon 9 died: 3 of erysipelas, 3 of purulent infection, 2 of pleurisy, 1 of hospital gangrene; 20 are said to have left the hospital cured, leaving 16 of which no further result is stated.

These tables appear to us to be valueless for want of accurate and minute investigation of the structure of the growth removed, as well as from the insufficient details relating to the progress and termination of the cases.

The work concludes with some brief details in reference to the diseases which affect the mammilla of the male, and the rudimentary organ of the infant.

John Birkett.

Review IV.

Klinik der Geburtshilfe und Gynækologie. Von Med. und Chir. Dr. J. Chiari, Med. und Chir. Dr. Carl Braun, und von Med. und Chir. Dr. J. Spaeth.—*Erlangen*, 1853.

Clinical Observations on Obstetric Medicine. By Drs. Chiari, Braun, and Spaeth.

The book whose title we have given above, the result of the labours of celebrated German physicians, is now on our table, and we propose giving our readers an insight into its contents. There is much in it which has been well known, and equally well described, by writers of our own country; but we are bound to acknowledge that, in the obstetric department of our profession, we are behindhand in works of easy reference and practical utility. Our systematic treatises on the science and art of midwifery are probably the best of their kind in any language, and leave scarcely anything to be desired in their particular sphere; but we possess no dictionary of midwifery, like our dictionaries of medicine and surgery; and the clinical reports published from time to time in our journals do not compensate the busy and anxious general practitioner for the want of such works on practical midwifery as are published by the Germans. We should hail with pleasure the appearance of a work on midwifery in our own language, on the principle of Busch and Moser's 'Handbuch der

Geburtskunde,' containing a good practical article on each subject connected with obstetricy, written by our best authorities, and arranged in alphabetical order; it would prove a boon to students, and form an acceptable book of reference to such as are engaged in the toilsome and anxious labour of midwifery practice. In the volume before us, numerous subjects are treated of in a very creditable manner, and it is somewhat difficult to make a selection. Hæmorrhage occupies an important part; and as there is one form of it which has rarely been described, and of which four cases are related, we transcribe one of them for the information of our readers.

"N. N., æt. 26 years, was delivered of a female living child of the usual size, after a natural and regular first labour. Soon after the expulsion of the placenta, which took place naturally, the patient complained of pain in the genital organs, which, becoming more severe, led to an examination, and to the discovery of a large, firm swelling in the vagina, supposed, from the manner of its origin, to be a thrombus. In order to prevent its increase, cold applications were made use of; but notwithstanding this, it had increased so much the next day, as to fill the cavity of the pelvis, the right labium being swelled to the size of a fist, and the swelling extending beyond the perineum backwards into the right buttock. The uterus was pushed upwards, and the pressure upon the bladder was sufficient to require the use of the catheter. The following day the swelling became of a violet colour, and on the third day it burst, about an inch behind the entrance into the vagina, and a good deal of coagulated blood was removed by the fingers. Meanwhile, all the symptoms of a severe metro-phlebitis appeared, and the large irregular cavity of the thrombus soon secreted very offensive matter. The patient died on the seventh day. The treatment of the thrombus consisted in cold applications during its formation, and frequently-repeated warm injections into its cavity for the removal of a foul discharge, after it had opened. Dissection exhibited the usual results of *endometritis septica* and metro-phlebitis; and between the vagina and pelvis, the irregular cavity of the thrombus, extending along the psoas upwards towards the right kidney. The surrounding parts, and even a portion of the mesentery, were stained of a violet colour by the sanguineous effusion."

This disease, or rather this accidental complication of labour, has rarely been noticed by authors, and must be of very infrequent occurrence. It is said to originate in the rupture of large vessels, but whether arteries or veins is a matter of doubt, and perhaps of no great practical importance to be known. The period at which it has been observed to take place is either during or after labour; and most frequently in first labours, when the head has been forced down quickly, and the vessels have not had time to accommodate themselves to the increased quantity of blood they have to contain, and therefore give way. It is remarked, however, that individuals subject to varicose veins around the vagina are not thereby predisposed to this accident. The thrombus generally makes its appearance in some part nearest the genitals; indeed, most frequently in the cellular membrane between the vagina and pelvis, near the rectum, labia, and perineum, and is discovered about the period when the afterbirth is expelled; for it requires some little time to develop itself. The extravasation sometimes extends outwards over the back and thighs, and internally to the peritoneum, and along the psoas muscle towards the kidney, but seldom into the texture of the uterus. The diagnostic signs are, the sudden appearance of a swelling during labour, more or less firm to the touch, and increasing in size during the pains; the pain belonging to the disease itself, if any, being obscured by the pains of labour. The bluish

colour of the skin does not appear until the effusion has made its way nearly to the surface.

When the extravasation is of slight extent, it may be absorbed; or the disease may result in the gradual escape of the extravasated blood by a spontaneous or artificial opening, with subsequent suppuration, terminating either in a cure, or pyæmia, and death. The treatment consists in endeavouring, in the first place, to prevent the increase of the tumour, as soon as it has been discovered. Compression of the swelling with cloth dipped in cold water has been recommended, but unfortunately can seldom be carried into effect, from the peculiar situation of the thrombus; and if pressure be applied externally without closing the bleeding vessels, matters may be made worse, by forcing the blood internally under the peritoneum. When cold and pressure cannot be applied directly upon the bleeding point, our treatment has to be limited to cold lotions, injections, and the introduction of pieces of ice into the vagina, in order to promote the coagulation of the extravasated blood. If a thrombus occurs during labour, and causes obstruction to the passage of the child, we are advised to hasten delivery, and if necessary, to open the tumour. If the tumour does not soon give way spontaneously, we are recommended to persevere with the cold applications for several days, and not open the cavity before the vessels are closed by coagula; but afterwards it may be opened, some of the coagulated blood removed, and a healthy suppuration promoted by warm injections and fomentations. Should the cavity be slow in filling up, weak astringent injections may be employed.

The volume before us also contains an elaborate article on the nature and treatment of convulsions occurring in the generative period of women, either during pregnancy, in the various stages of labour, or after delivery, by Dr. Carl Braun. The results of his own experience, in the observation of 52 cases of convulsion occurring in 24,132 labours, have led him to differ somewhat from former prevailing opinions. He arranges the cases as follows, according to their causes:

"A. Convulsions occurring without the co-existence of morbus Brightii.
1. Hysterical convulsions.
2. Epileptic ditto.
3. Convulsions from cerebral diseases—as hyperæmia, apoplexy, meningitis, encephalitis.
4. —— from the inhalation of irrespirable gases, or from the effects of poisonous substances.

"B. Convulsions which are connected with Bright's disease—as uræmic intoxication, under the name of eclampsia.

"C. This division includes those cases where, notwithstanding the presence of Bright's disease, the act of labour occurs normally, and, with the exception of certain disorders of the senses during pregnancy—as amaurosis, amblyopia, hemeralopia, deafness, &c.—no other functional disturbance is produced.

"D. Natural parturition during the existence of Bright's disease, *without convulsions*."

Several writers in this and other countries have remarked upon the presence of albuminuria in connexion with puerperal convulsions, but the subject is still in considerable obscurity, and is of sufficient importance to demand further investigation. Dr. Carl Braun has thrown new light

upon the influence of uræmic intoxication in the production of eclampsia; and although it must be admitted that some of his propositions require further testing before they can be fully accepted, we think it worth while to place before our readers such an analysis of his interesting paper as will induce them to devote their attention to the practical questions involved. To be enabled to distinguish clearly between the various kinds of puerperal convulsions, and at once decide upon the befitting treatment, is a most desirable qualification for an obstetric practitioner; for a mistake in the application of remedies in a disease of such severity would often, probably, be attended with fatal consequences; and when we consider that, with a train of external phenomena very similar in appearance, one case may demand venesection to as great an extent, perhaps, as almost any other disease whatever, whilst another requires an exactly opposite method of treatment; when, too, the nature of the symptoms is such, that delay in determining upon the proper management of the patient may lead to the most serious consequences, we cannot but hail with pleasure every philosophical or scientific attempt to elucidate the causes, diagnosis, and prognosis, of the different kinds of puerperal convulsions.

It has generally been supposed that the presence of albumen in the urine of pregnant women has been due to congestion of the kidneys, from the pressure of the enlarged uterus; or to a general congestion of the system during the pregnant state, often producing anasarcous swellings, or effusions into serous cavities. Cases of this description are familiar to us all; but it will be found that, in some cases, disease of a more serious character is at the bottom of it all, and that in such our prognosis, as concerns the safety both of mother and child, must necessarily be of a much more unfavourable character. We now proceed to describe the symptoms of eclampsia, or uræmic convulsions, according to our author.

Symptoms.—Eclampsia, or puerperal or uræmic convulsions, occur suddenly at some period of the generative process, and consist of a more or less regular succession of phenomena, amongst which loss of consciousness is the most prominent. During the attack, the head and neck appear swollen, red, and livid; the eyes move rapidly in various directions, or remain fixed in their orbits with a vacant stare; the conjunctivæ are generally injected, the tongue protruded, and frequently wounded by the teeth, so that bloody froth escapes from the mouth; it is sometimes, also, considerably swelled. The muscles of the face are convulsed, and the extremities are affected with rapid movements of flexion and extension, alternating with each other. The whole trunk is also thrown backwards and forwards, or else immovably and stiffly bent backwards, or to one side. The carotids pulsate strongly; the jugular veins become distended; the respiratory muscles, especially the diaphragm, are in a state of spasm, threatening suffocation. Generally, there is vomiting, and the urine and fæces are passed involuntarily. The skin is covered with perspiration, and its temperature altered. Reflex sensibility is arrested during the attacks. The pulse may be either frequent or slow.

After these symptoms, an interval of variable duration follows, in which the patient lies in a soporose condition, with extended, stiffened limbs, difficult, frequent, and stertorous breathing, and continued loss of sensation and consciousness. The duration of the attack may be a quarter

or half an hour, or a whole day, the coma remaining uninterruptedly, so as to simulate a severe attack of apoplexy.

Should the first attack not terminate fatally, a remission ensues; the breathing becomes less impeded, the muscular rigidity disappears, consciousness sometimes returns, but is often imperfect, and the frequency of the pulse diminishes. The abdomen seems tender to the touch, and reflex sensibility, during the intervals, is generally exalted. The patient mostly complains of confused, dull headache, and great exhaustion; and this continues until a degree of restlessness, stretching of the limbs, convulsive movements of the eyelids, and turgescence of the features, announce a fresh attack. In this manner, the paroxysms are repeated often in the day, sometimes from ten to fifty times, a state of unconsciousness remaining during the intervals. The outbreak of the paroxysm itself is occasionally preceded by precursory symptoms, such as headache, vertigo, heat of skin, hallucinations, scintillations, imperfect vision, ringing in the ears, difficult articulation, mental irritability, pain in the præcordial region, nausea, vomiting, irregular pulse, and considerable lassitude without any apparent cause.

The paroxysms generally occur either in the latter months of pregnancy, or during or soon after labour; they are commonly preceded, sometimes for several weeks, by œdematous swellings in various parts of the body, although this is not always the case. The œdema occurs less frequently in the upper half of the body than in the ankles and labia; but in the latter months it is sometimes diffused over the whole body. With œdema of the face there is generally more fever, suffusion of the eyes, and redness of the cheeks, so that the countenance assumes a bloated appearance. These dropsical swellings are very uncertain, sometimes diminishing, or even entirely disappearing towards the completion of labour; whilst the quantity of albumen in the urine, and the disease of the kidneys themselves, may be on the increase. The skin of those parts of the body which are not œdematous is dry, and frequently assumes a chlorotic appearance, its temperature being at the same time reduced. The œdema which is connected with albumen and fibrinous cylinders in the urine stands in intimate relation with eclampsia. Anasarca of the lower limbs and hydramnios are frequently met with without a trace of albumen or casts of tubes in the urine, and are then never accompanied with eclampsia during the several periods of pregnancy or labour.

In eclampsia the urine is generally acid, always deposits, with nitric acid and heat, a large quantity of albumen, and exhibits under the microscope fibrinous cylinders, and often also blood corpuscles. The quantity of albumen corresponds with the extent and duration of Bright's disease; and increases in the latter months of pregnancy, and with the attacks of eclampsia.

The normal proportion of the constituents of the urine undergoes the following alterations, viz.:

The uric acid is diminished.

Urea diminished or almost wanting.

Chlorides but little altered.

Sulphates and phosphates either diminished or increased.

Uroxanthin increased.

The specific gravity of the urine varies from 1010 to 1030. If the sediment of the urine be examined in the first 24 hours, blood and mucous corpuscles and epithelial scales will be found, but these disappear when decomposition ensues. The more acute the disease of the kidney, the more cloudy the urine, and the greater the number of blood-corpuscles. There is frequently also increased sensibility of one or both kidneys under pressure externally, and sometimes various affections of the digestive organs. Eclampsia often occurs in first labours, but seldom recurs in future ones.

As neither time nor space will allow us to enter fully into the consideration of each division of Dr. Braun's interesting article, we shall, on the present occasion, content ourselves with presenting to our readers a recapitulation only of the views he entertains upon the subject.

1. Convulsions in females during the generative period, depend either upon hysteria, epilepsy, diseases of the brain, mineral or vegetable poisoning, or upon uræmic intoxication.

2. The most frequent cause of eclampsia is uræmia and Bright's disease.

3. Chronic hysteria, and epilepsy, exert no injurious influence upon pregnancy and labour, or upon the life of the fœtus; they are never connected with Bright's disease, nor are fibrinous cylinders or a large quantity of albumen found in the urine.

4. Primary diseases of the brain, as apoplexy, meningitis, &c., are rarely causes of convulsions; and when they occur simultaneously with Bright's disease, they are the consequence, not the cause, of the convulsions.

5. All forms of convulsion, with their different causes, may occur without pregnancy.

6. They may also occur in males from any of the above-mentioned causes, except hysteria.

7. Amongst the more important causes of Bright's disease in the pregnant, may be considered venous congestion of the kidneys from the pressure of the enlarged uterus and distended abdomen, as well as the sanguineous diathesis often connected with pregnancy.

8. In *nephritis diffusa,* urea becomes detained in the blood, is converted into carbonate of ammonia, and then excites convulsions.

9. In cases of Bright's disease during pregnancy, if carbonate of ammonia be found in the blood, a speedy outbreak of convulsions may be expected.

10. No convulsions, however, occur in Bright's disease in the pregnant, provided but a small quantity of urea accumulates in the blood, or does not become converted into carbonate of ammonia.

11. The strong action of the uterus during labour is not the cause of this conversion, or of the uræmic convulsions; for these occur nearly as frequently without labour, during pregnancy and in the puerperal state; and sometimes in those who are not pregnant.

12. The abortions which frequently occur in uræmic convulsions are the effect of the uræmia, not the cause of the convulsions.

13. Eclampsia—i. e., uræmic convulsions—has no immediate connexion with the pains or process of labour.

14. The albuminuria cannot be the result of congestion occasioned by the convulsions, for it precedes the uræmic convulsions for days or weeks,

and does not, as a rule, occur under other forms of convulsion, as from hysteria, epilepsy, &c.

15. Attacks of eclampsia *during pregnancy* may cease, and the albuminuria continue.

16. But if the attacks cease *after labour*, and there be present only a slight degree of Bright's disease, the albuminuria disappears after a few days; but continues for a longer time if the disease of the kidney be in a more advanced stage.

17. The diminution in the size of the uterus from the completion of labour contributes very much to the disappearance of albuminuria, when there is neither fatty degeneration or atrophy of the kidneys.

18. Bright's disease (without convulsions) never gets well during pregnancy; after labour it mostly disappears within a much shorter time.

19. Albuminuria will be found in all cases of eclampsia not dependent upon hysteria, epilepsy, primary cerebral diseases, or poisoning.

20. Epilepsy may take place simultaneously with Bright's disease and uræmic convulsions; and Bright's urine may be found in an individual affected with habitual epilepsy.

21. With a frequent repetition of uræmic convulsions the fœtus dies, through the injurious influence of blood impregnated with carbonate of ammonia. The life of the fœtus is not compromised by the mechanical effects of convulsions which depend upon hysteria, epilepsy, or diseases of the brain.

22. Examination after death from uræmic convulsions always discovers Bright's disease of the kidneys; more frequently in the primary than in the latter stages (of fatty degeneration and atrophy).

23. Œdema and anæmia of the brain are as frequently found after death from eclampsia, as hyperæmia and consecutive apoplexy.

24. Reflex sensibility is entirely abolished during each uræmic attack, but during the intervals loss of consciousness generally increases.

25. The injurious effects of venesection generally in eclampsia have been observed by Kiwisch, Litzmann, Sedgwick, Blot, and King, and the uncertainty of the practice has been confirmed by our own experience. In cerebral eclampsia, however, venesection is a proper remedy.

26. Chloroform inhalations are the best means of mitigating and bringing to an end uræmic convulsions, either during pregnancy, labour, or in the puerperal period.

27. The diuretics most to be relied upon for the relief of uræmia and Bright's disease are the benzoic, citric, and tartaric acids.

28. Bright's disease during pregnancy admits of relief only, not of cure.

29. Lessening the size of the uterus, and removing the child, are the most effectual means of curing the affection of the kidneys and uræmic intoxication.

30. Exciting and hastening labour by artificial means diminishes the danger both to mother and child arising from uræmic convulsions.

31. Artificial premature delivery is not, as a rule, to be resorted to in Bright's disease; but is to be recommended in actual uræmic convulsions.

32. The most appropriate method of producing premature labour, and

hastening delivery in cases of uræmic eclampsia, is by forcibly dilating the vagina by means of a caoutchouc apparatus (*Colpeuryse*).*

We trust we have thus introduced to the profession sufficiently, though imperfectly, the more recent views entertained by our German *confrères*. We shall, perhaps, return to the subject at some future time, as well as offer a few observations upon other articles contained in the volume before us. Meanwhile, we would strongly urge upon our brethren the more accurate study of puerperal convulsions, aided as they now will be by the glimpse we have afforded them of what is most novel in the investigation.

E. Copeman.

Review V.

Beiträge zur Vergleichenden Pathologischen Anatomie der Gelenkkrankheiten. Von Dr. E. Gurlt, Prakt-Artze und Assistenz-Artze der Chirurgish-Augenartzlichen Klinik der Universität zu Berlin. 1853.

Contributions to the Comparative Pathological Anatomy of Diseases of the Joints. By Dr. E. Gurlt.

(*Continued from No.* 26, *p.* 359.)

We proceed with the review of this well-compiled and highly instructive work, confining our remarks on the present occasion to the pathological anatomy of the joints of the superior extremity. The information, however, collected and arranged by Dr. Gurlt is in so condensed a form, that the perusal of the work itself can alone do justice to the author: and we recommend it most strongly to those familiar with the German language.

Diseases of the Clavicular Articulations.

If it were true that constant motion, the support of a weight, and exposure to cold, were sufficient to determine in joints the relative frequency of disease, we should expect to find its traces nowhere more often than in the acromio and the sterno-clavicular articulations; whereas, the opposite is the case. Indeed, the sternal joint is, according to Gurlt (p. 233), the most rarely affected of any articulation in the body. Guy's Hospital Museum contains a specimen (No. 1092) of complete bony anchylosis of the clavicle with the sternum, history unknown; but there is none such in the museum of St. Bartholomew's; and the cases of resection upon record, either resemble that of M. Davie, who operated to relieve the œsophagus from pressure caused by displacement of the clavicle consequent upon distortion of the vertebral column; or that of Dr. V. Mott, of New York, who took away the entire bone for osteo-sarcoma (i.e., osteo-cartilaginous growth). We have before remarked upon the importance of the extent and relations of the synovial membrane; and upon the favourable influence exerted as regards the production of disease, by an interarticular fibro-cartilage, which subdivides the joint, and affords

* "Of the various means of producing labour artificially in cases of uræmic convulsions before dilatation of the parts has taken place, we prefer an invention of our own consisting of a tent (*tamponade*), made with a bladder of caoutchouc, by means of which strong pains are excited, and the wished-for dilatation of the orifice is effected entirely without danger." (p. 347.)

under all circumstances a soft and well-fitting pad, against which the bones may press easily in every movement.

Chronic Inflammation of the Acromio-clavicular Articulation occurs usually, but not always, associated with similar disease in the shoulder-joint. The acromial end of the clavicle is looser than natural, and rises from its connexion with the scapula, as if it were a case of partial traumatic luxation. The articulating surfaces are deprived of cartilage, broad and flattened, and surrounded by osteophytes. The capsule is roomy and loose; the fibrous tissue is thicker than natural, and fibro-cartilaginous growths often project from its inner surface. Gurlt refers to cases related by Hamilton Lebatt, R. Adams, and E. Canton. He quotes the particulars, also, of two preparations; one from the Museum of the Royal College of Surgeons of England, the other from the Anatomical Museum of the University of Berlin. In the former, the corresponding articular surfaces of the clavicle and scapula are enlarged and rough, and perforated by numerous small round openings; the margin of the joint is surrounded by new bony growths, in the form of hard, knotty masses. The opposite acromio-clavicular articulation was similarly diseased. In the latter, a case of chronic inflammation of the shoulder joint, the clavicular end of the acromion is thickened; bony knots surround the articulation; the cartilage is absorbed, and the opposed osseous surfaces are polished; the capsule is in its normal state. We have dwelt upon this subject because we have, from examinations in the dissecting room, ascertained the frequency of the disease, and the great tendency among those who give the parts but a cursory examination to refer the morbid appearances to external injury. Simple dislocation of the acromial end of the clavicle is not very common, but is easily diagnosed, and, as far as we have observed, admits of ready cure under proper treatment, leaving no deformity of the bones.

The Shoulder Joint.

When the diseases of the shoulder joint are considered generally, it is found that the different varieties of inflammation are relatively rare; but that the consequences of external injuries are more frequent than in other articulations of similar size. Synovitis, according to Gurlt, is the most uncommon; then follows chronic inflammation, with ulceration; then gouty deposits, which are found only when other joints are similarly affected; cancerous growths are excessively rare; hydatids may be regarded as a curiosity. But there are many kinds of congenital luxation, and frequent specimens of traumatic luxation; although it is necessary to distinguish between the effects of chronic disease and cases in which, after accident, the bones have not been replaced during life. Gurlt thinks it doubtful whether the long tendon of the biceps can be displaced from the inter-tubercular groove solely as a result of accident, in which opinion we agree. Fractures of the upper extremity of the humerus are common; those of the scapula are most rare. We shall proceed to examine, in the first place, the different inflammatory affections.

Inflammation of the synovial membrane usually ensues as a consequence either of rheumatism or of general purulent infection. Bonnet mentions

the particulars of two cases of the former variety: 1. *Chronic inflammation of the scapulo-humeral articulation; absorption of the cartilages; ulceration of the articular surfaces.* 2. *Chronic rheumatism of the shoulder, with absorption of the cartilages.* The effects of acute rheumatic inflammation in the shoulder-joint are precisely similar to those witnessed in similar structures in other parts of the body: the synovial membrane acquires a red hue from the congestion, in its entire extent, of its bloodvessels, which abruptly cease at the margin of the articular cartilages. The cavity is filled with a whitish homogeneous pus, and similar changes take place in the contiguous bursæ. We believe that this is, at least, one form, and perhaps the principal, of that disease, which ultimately involves every articular structure, and which Gurlt (op. cit.) describes under a distinct head; for, as the symptoms proceed, the fibrous capsule becomes thickened, and perhaps contracted and perforated, by fistulous openings; the subcutaneous tissue is infiltrated by a gelatino-lardaceous substance; the surrounding muscles waste, and the integument, red and shining, presents numerous ulcerated openings, leading through the capsule to denuded bone. The spongy texture of the humerus, exposed by being deprived of its cartilage, is filled with a fibrinous or purulent exudation, and frequently there is absorption or destruction of the opposed articular surfaces, producing irregularities, depressions, and vacuities. It is obvious that these appearances ensue from inflammation extending to the bone, for purulent infiltration has been noticed by Löbl* in the coracoid process, and adjacent parts of the scapula. In the head of the humerus, cavities have been found, filled by pus,† or by pus and necrosed bone;‡ and osteophytes, of different forms, become developed around the whole circumference of the affected joint, generally limited to the neck of the scapula and of the humerus, but occasionally extending to the coracoid process, and the acromial end of the clavicle.

Collections of matter within the joint follow the course of the long tendon of the biceps, or the tendon of the subscapularis, both of which are invested by a prolongation of the synovial membrane. In the first case, the pus presents at the anterior border of the deltoid muscle; in the second, behind and below the shoulder, between the muscle and the subscapular fossa. In other cases, the matter bursts into the axilla, or runs along the course of the upper arm, governed by peculiarities in the case. The neighbouring bursæ become similarly affected, but Gurlt has met with no instance of consecutive dislocation of the humerus.

We repeat our belief that, from the history and symptoms characterizing these cases, the disease usually commences in the synovial membrane, and spreads thence, in its onward progress, to other structures—namely, the fibrous membrane and the bone. Cases are upon record where, in other regions of the body—the knee or ankle—inflammation has spread from the bone towards the interior of the joint, but, in such instances, there is usually an absence of that steady progress from bad to worse, which marks the disease of the shoulders. A painful condition of the limb near the joint, accompanied by swelling of the bone, is fol-

* Zeitschrift der K. K. Gesell. der Aertze zu Wien, 1844, Band I. § 161.

† Bonnet: Maladies des Articul., tom. ii. p. 579.

‡ Meinel: Prager Vierteljahrsschrift der prak. Heilk., 1852, Band iii. § 68.

lowed, at some uncertain period, by rapid extension of disease to the articular cavity, into which one or more pieces of hardened and dead bone are thrown from the inflamed cancellous texture. The immediate symptoms are, pain of the most agonizing character, severe constitutional disturbance, under which the patient might sink, were relief not afforded by amputation. The joint, when opened, exhibits destruction of the cartilage, thickening of the synovial membrane, and softening of the ligament; and the cavity contains fragments of bone, soaked in pus, often fœtid, and sometimes discoloured. " Such is not the usual history preceding the cases of disease of the shoulder-joint. The progress of the symptoms indicates distension of the synovial membrane, thickening and tenderness of the capsule, and, subsequently, ulceration of the cartilage, and the exposure of the inflamed bone, when abscesses form and burst externally, leaving fistulous passages leading to the morbid parts.

The principles of treatment laid down by Bonnet* in acute arthritis, or rheumatic synovitis, are such as are usually pursued: loss of blood when the congestion is active; narcotics when the pains are severe. In general, one commences by the employment of leeches, emollients, and narcotics; in a more advanced stage, mercurial frictions and different kinds of poultices are employed, according to the case; finally, blisters, or the pomade of nitrate of silver, are necessary. When the acute inflammation shows a tendency to pass into a chronic state, gradual and passive motion should be substituted for perfect immobility, and, subsequently, compression; the douches, mercury, colchicum, and iodine, may be administered according to the symptoms.

But should the joint pass into that state last described, when the inflamed and denuded bones rub against one another in every movement of the arm, to the great distress of the patient, to whom such a limb is useless, the question arises, whether, by operation, the articulating surface of the humerus, or of the scapula, or of both, may not be advantageously excised. This joint, both as regards the conformation of the bones and the relations of surrounding parts, is favourably circumstanced for the operation of resection, and we should not hesitate to recommend it, although, perhaps, the results of cases are not quite so flattering as some authors have maintained. In thin subjects, the head of the bone may be reached by a simple incision; but in more muscular persons, the formation of a flap is necessary. Moreau made it quadrilateral, the attached part, or the base, being downwards; Manne, quadrilateral, the base being superior; Sabatier, triangular, the base superior—he even removed it altogether; Morel, semilunar, the base superior; Syme makes first a longitudinal incision, of three to four inches, through the middle of the deltoid; then, from the lower extremity, he carries another to the posterior part of the axilla. The head of the humerus can be easily raised, and removed with a saw; the articular surface of the scapula can be also taken away when necessary; and certainly it is important to act as much as possible upon the diseased structures, for, should aught remain to impede the fibrous union of the deeper parts, and the closure of the external wound, the joint becomes the seat of frequent inflammations, abscesses form and burst, leaving fistulous passages, the skin assumes a

* Traité Thérapeutique des Articulations, p. 154.

red hue, the subjacent parts are œdematous, and the limb is perfectly useless.

We do not, however, purpose, in the present review, entering upon the question of treatment; our limits confine us to the point of pathological changes.

Chronic Inflammation of the Shoulder Joint.—Until recently this disease has been confounded with partial luxation of the humerus; it is now ascertained that the number of instances of the latter condition is much fewer than formerly supposed, and to two of our countrymen—namely, Mr. R. Adams* and Mr. Edwin Canton,†—is due the merit of having put this subject in its proper light. This disease, probably, does not commence in the synovial membrane. The principal changes are those which the osseous parts of the joint undergo. The upper arm becomes enlarged, but not equally so, inasmuch as the tubercles and the articulating surfaces show increase of substance: the latter are flattened, and become extended, so as to involve the former as well as the upper part of the inter-tubercular, or the bicipital groove. Upon examining the head of the bone we may find it covered partially or entirely by cartilage, which has undergone fibrous degeneration, converted into an irregular knotty eminence, or perfectly smooth, denuded of cartilage, and eburnated. All these conditions may be met with in the same specimen; the polished surface being confined to those parts where constant friction has gone on during life. Numerous bony growths, osteophytes, are developed around the articulation, where they tend to limit and restrain the free movements of the limb. When the head of the bone rests partly in the normal cavity, and partly in a new one below, it is marked by a transverse groove, formed by the pressure of the lower margin of the glenoid cavity.‡ The glenoid cavity is usually enlarged; it acquires twice its normal extent; loses its oval form, becoming circular, and the glenoid ligament disappears mostly at the inferior border.

In another class of cases, the articular surface of the scapula becomes extended upwards and inwards; the superior surface, separated from the lower by the remains of the normal margin of the glenoid cavity, extends as high as the acromion and the coraco-acromial ligament; and fibrous bands pass between the contiguous structures.

Smooth surfaces are constantly found on the coracoid process, the acromial end of the clavicle, and the acromion, which in some cases is hypertrophied, and divided along the line of its epiphysis into two, thus resembling a fracture. These are the morbid changes which precede the displacement of the head of the humerus, generally upwards under the acromion, or inwards under the coracoid process. More rarely, the displacement is backwards, on the costa of the scapula, where the new cavity is then formed, or downwards into the axilla. It can be understood from this description how easily such cases might be mistaken for non-reduced luxations; and the idea would receive further support from the examination of the interior of the joint. The long tendon of the biceps is rarely

* Todd's Cyclopædia of Anat. and Phys., Art. Shoulder Joint.

† London Medical Gazette, vol. iv., 1848, p. 410.

‡ Bonn: Commentatio de Humero Luxato, Lugd. Bat. et Amstelod, 1782. Hargrave: Edin. Med. and Surg. Journal, vol. xlviii. 1837. Sandifoot: Mus. Anat., Tab. 99, fig. 1—4, p. 222.

in its integrity; when whole, it is often pushed from the bicipital groove, flattened and spread out, and separated into three or more strings. But generally, this part of the tendon is absorbed, and the corresponding belly of the biceps muscle is atrophied. The fibrous capsule is thickened and hypertrophied (in some cases it is thin), being usually more capacious than natural, having acquired attachments corresponding with the increased extent of the articular cavity. The synovial membrane is converted into a dark-red, or reddish-brown substance, having a villous appearance, and containing bloodvessels and fat. In it we often find those fibro-cartilaginous bodies which become ultimately loose in the joint. The bursa mucosa subdeltoidea is often morbidly distended,* and occasionally communicates† with the synovial cavity. The surrounding muscles waste, their tendons becoming blended with the fibrous capsule. The tendons of the supra spinatus, infra spinatus, teres minor, and subscapularis, may separate from their tubercles; and with these changes, not uncommonly simultaneously affecting both the right and the left sides, the acromio-clavicular articulations present those changes mentioned in a previous section.

The determination as to whether a case be one of chronic inflammation of the shoulder joint, or of non-reduced dislocation, is settled‡ by the examination of the head of the humerus. If we use the information deduced from a consideration of what occurs in the hip joint, where, from the shape of the acetabulum, no confusion can occur, it will be found that the head of the dislocated femur becomes in time atrophied and smaller, not enlarged and flattened; and hence the consolidation of the head of the humerus, its increase in size, the eburnation of its surface, and the development of osteophytes, give evidence, conclusive to the minds of most, of chronic inflammatory action. The opinion is further confirmed, when it is seen that similar changes have occurred symmetrically in both shoulders; for experience teaches us, that double traumatic luxation is of very rare occurrence, and not necessarily followed by changes in the bone. Scattered through the journals are notices of such accidents, but they are quite exceptional.§

The Dislocation of the long Tendon of the Biceps from the inter-tubercular groove, as described by Soden, is referred by Gurlt to the changes consequent upon chronic inflammation. Upon this point further observations are requisite. The rarity of fracture of the neck of the scapula is properly insisted upon. The fractures of the head of the humerus he arranges in the following table:

"A. Fracture through the Anatomical Neck. (Intra-capsular Fracture.)

a. Fracture without impaction.

α. Simple fracture through the anatomical neck.

β. Fracture through the anatomical neck, with luxation of the upper arm.

* Adams: op. cit., p. 595.

† Gregory Smith: Lond. Med. Gaz., vol. xiv., 1834.

‡ Gurlt, p. 275.

§ Coote: Hospital Reports in Lancet and Med. Times and Gaz., 1854.

b. Fracture with impaction.

α. Simple fracture through the anatomical neck, with impaction.

β. Fracture through the anatomical neck, with impaction and separation of the tubercles.

γ. Fracture through the anatomical neck, with *inversion* of the upper fragment, and impaction.

"B. Separation of the Greater Tubercle.

"C. Separation of the Lesser Tubercle.

"D. Separation of the Epiphysis from the Diaphysis (in children), or fracture through the former line of separation (in adults). Extra-capsular fracture.

a. Separation of the epiphysis.

b. Fracture through the ossified line of union.

"E. Fracture through the Surgical Neck.

a. Fracture without impaction.

α. Simple fracture through the surgical neck.

β. Fracture through the surgical neck, with dislocation.

b. Fracture with impaction, the lower portion being driven into the upper.

Gurlt remarks, that in the intra-capsular form of fracture, *without impaction,* osseous union of the fragments is not to be expected; basing his view on the observation of Mr. Wilkinson King,* that in intra-capsular fracture the nutrient artery of the head of the bone is separated from the part which it is destined to supply. In three of the cases, however, which he has himself recorded, osseous union had taken place: in one only was the fracture united by a fibro-cartilaginous substance. In the other cases there appears to have been no attempt at repair. Whether impaction takes place or no, it is the lower fragment, without doubt, that undertakes especially the office of producing callus. On the whole subject, however, of fracture *with impaction,* special observations are still wanting.

A single case of fracture of the anatomical neck of the humerus with impaction, and separation of the greater tuberosity, is recorded by R. W. Smith.†

From the same author are quoted two other cases‡ of a very remarkable variety of fracture; where the head of the humerus, after complete separation from the shaft, has become completely *inverted,* so that the broken end came to lie in the cavity of the joint, and the articular surface on the fractured extremity of the shaft. In this position, the head of the humerus seems to have become impacted in the spongy portion of the bone on which it rested, and to have occasioned a new fracture, breaking off both tubercles and a portion of the bicipital groove. However, the nature of the second of the cases recorded seems to be in a great measure conjectural.

Separation of the greater tubercle (a very rare form of fracture) is necessarily attended by luxation; the fragment itself being forcibly drawn into the articular cavity. In an interesting case recorded by Mr. Hilton,§ the head of the humerus had escaped from the capsule, and lay

* Guy's Hospital Reports, vol. ii., 1844, p. 350.

† Smith on Fractures and Dislocations, p. 191.

‡ Loc. cit., p. 193. § Guy's Hospital Reports, vol. v., 1847, p. 93.

external to it, in a smooth cavity, hollowed in the free surface of the separated tubercle, with which, therefore, it articulated. The surfaces of this new joint are said to have been all invested with a synovial membrane.

A single and uninteresting case of separation of the lesser tuberosity is given by Thudicum.*

The separation of the epiphysis from the diaphysis in children, as well as fracture through the line of former separation† in adults, offer no points for special consideration; nor do the author's remarks on fractures of the surgical neck of the humerus present any features either of novelty or interest. When such fractures are attended by impaction, it is the *lower*, not the *upper*, fragment which is driven into the other; a circumstance which, according to the author, exercises no little influence on the subsequent osseous reunion of the bone.

Of fracture of the anatomical neck of the humerus with luxation, but without impaction, seven cases are enumerated. The head seems usually to be dislocated into the subscapular fossa; in a preparation in the Museum of St. Bartholomew's Hospital,‡ it had formed for itself a new articular cavity in this situation, in a mass of bone which had been deposited around it. In one case,§ (Lenoir's) the luxation took place on to the third rib. The shaft of the bone is ordinarily forced upwards towards the glenoid cavity, with which it forms more or less intimate connexions. In one of Sir A. Cooper's cases,‖ the fractured extremity was wedged immovably into the articular cavity; in two others¶ it was so rounded off and polished as to form a sort of articulating surface, retained within the cavity by newly formed fibrous bands.

Of simple fracture through the anatomical neck of the humerus with impaction, Gurlt contributes no information beyond that afforded by English surgical literature. He quotes (p. 289) Smith's** case of a female, aged 52, in the shaft of whose humerus the articular head was so deeply imbedded between the tubercles, that it was below the level of the tuberculum majus. This latter process was broken off, dislocated outwards, and formed an obtuse angle with the external surface of the diaphysis. All the lines of fracture were united by bony substance. To this author we should refer our readers for information upon this point, as well as upon "Fracture of the anatomical neck with twisting of the upper fragment with impaction;" fracture and separation of the tuberculum majus and minus. Of separation of the epiphysis from the diaphysis a case is recorded by Reichel.††

Of fracture between the epiphysis and diaphysis (i.e., of the anatomical neck), of fracture of the surgical neck, with separation of the fragments, or with impaction, we need say but little. Upon these points literature is chiefly indebted to the labours of British and Irish surgeons, among whom the name of Sir Astley Cooper still stands prominent. It is true that we may not be able to diagnose these different forms of accident during life; that we may include them for practical purposes under the one head of "fracture of the humerus near the shoulder." But a proper

* Loc. cit., § 22. † Sir A. Cooper: Guy's Hospital Reports, vol. iv., 1839, p. 277.
‡ Series III, Subseries C, No. 103. § Gaz. des Hôpitaux, 1851, No. 72.
‖ Mus. Coll. Surg., No. 875. ¶ Guy's Hospital Reports, vol. iv., 1839, pp. 273—279.
** R. W. Smith on Fractures. †† Diss. de Epiphys. ab Ossium Diaphysi Diductione, 1759.

knowledge of what may occur never can be otherwise than valuable in doubtful cases, while the fact of the possibility of impaction would deter the surgeon from making such violent and determined efforts to detect crepitus as were at one time considered necessary, though productive of intense pain.

The Elbow Joint.

The elbow joint is subject to the same diseases as other articulations. Simple synovitis is less common than that variety of inflammation, often of chronic character, which passes into ulceration of the articular cartilages. Hydatids have been observed by Gurlt; but cancer, he says, he has not witnessed here any more than in the shoulder. Gouty deposits occur, but in no very marked degree. Congenital irregularities of position affect the radius. There are occasional malformations; one instance of synostosis of the radius and humerus.* Luxations occur in great variety; and there are not a few specimens of united fractures. Finally, changes of considerable importance take place from long inactivity of the limb; namely, atrophy of the parts subjected to pressure, and of those from which pressure has been removed. In illustration of these points two cases are quoted from the 'Dublin Journal of Medical Science.'† In the first case, both elbow joints were in permanent flexion; the rotatory movement of the radius had been lost for many years. In the second, the changes occurred in an elbow, fixed in the extended position, following amputation of the fore-arm, performed many years previously. Here, too, there was no power of rotation, and the head of the radius was larger than the corresponding surface of the humerus.

Inflammation of the Synovial Membrane of the Elbow is pronounced by Gurlt a comparatively rare form of disease, because in many cases, where the joints were generally affected, this articulation escaped. He quotes, however, in illustration of his subject, the particulars of a preparation in the Museum of St. Bartholomew's Hospital (Ser. II., No. 12). "Chronic inflammation of the synovial membrane, of fifteen months' duration, in a man aged 60. The synovial membrane is converted into a light-coloured brown substance of firm texture, half to three-quarters of an inch thick, with white lines, which radiate in various directions: and it has a white smooth surface. The morbid change ceases at the border of the articular cartilage. Around the head of the radius, the thickened synovial membrane forms a fold, which projects into the joint; both cartilages and bones are healthy." We can assure the author that this disease is by no means uncommon in this country, especially among the young of the poorer classes. It is less common in adults, but even here it occurs under circumstances calculated to excite a subacute form of vascular disturbance.

In the investigation of an inflamed elbow joint in its advanced stages, there are found under the skin, ulcerated by fistulous openings, a lardaceo-gelatinous infiltration of the subcutaneous tissues; generally the swelling is upon the posterior part of the limb, and presents a contrast with the structures anteriorly. The fore-arm is mostly in the half-flexed and prone

* Bulletin de la Soc. Anatom., 1833, tom. ii. p. 82.

† R. Adams: Dublin Journal of Medical Science, vol. xvii., 1840, p. 506.

position. There are frequently numerous abscesses in the proximity. The articular extremities of the bones are diseased; the cartilage is absorbed; the bones themselves ulcerated; and examination of the cancellous texture shows that the inflammation commenced there, and spread towards the surface. In such cases there are generally necrosed pieces attached to a bone, at the end of a fistulous passage, or loose in the articulation; in others, osteophytes grow from the bones. When the disease is confined to the radial side of the arm, the infiltration is limited to this region; the ligaments become softened and elongated, or even destroyed, and the head of the radius itself may protrude through an ulcerated opening of the skin.* The same process may take place on the opposite side of the limb with the internal condyle of the humerus. When the greater sigmoid cavity becomes enlarged by ulceration, the end of the humerus appears to project forwards, the radius and ulna being drawn in an opposite direction by the triceps. Pus in the joints usually collects in the posterior part, where the synovial membrane is loose, and it bursts either on the outer or inner side of the triceps, generally at this former spot. When it collects in the anterior part, it makes its way under the brachialis anticus along the upper arm, and bursts, as in the preceding case, on the outer or inner side of the limb, so that from a consideration of the situation of the fistulous opening, it is not always easy to determine the course which the matter has taken. Repair may take place by bony anchylosis; complete, by the firm union of all the bones; or partial, as, for example, union between the olecranon and the humerus,† or the ulna and the humerus. Or the ends of the bone may suffer great loss of substance, and unite by their flat surfaces at a right angle. Upon the diseased bones of the elbow, the operation of resection has been performed with great success, chiefly by Mr. Fergusson, in cases were nature seemed unable to complete the process of osseous repair.

A Chronic Form of Inflammation leads to removal of the articular cartilages, to eburnation of the bones, and the formation of deep lines or grooves in its substance, corresponding with the direction of movement. Fibrous growths and osteophytes abound; and new ligamentous bands are developed, passing from bone to bone. The synovial membrane is thickened and vascular; the fibrous capsule is thickened. In many cases there are loose bodies in great numbers, of all sizes, from that of a pea to a walnut. Some yet retain an attachment by a narrow pedicle. The largest are usually found attached to the coronoid process of the ulna, where they are enclosed in a process of synovial membrane.‡ Some are quite bony, others fibrous or fibro-cartilaginous. The bursa mucosa olecrani may be morbidly enlarged. No treatment can restore the healthy condition of a joint thus affected, nor are the symptoms during life such as to demand surgical interference.

Luxations of the bones composing the elbow-joint are common, especially among the young; but considerable difference of opinion still exists as to their varieties and number. The following table, arranged by Gurlt, will be perused with interest by most surgeons.

* Lobstein. Traité d'Anat. Pathol.

† Sandifort: Mus. Anat., vol. iv., Tab. 160.

‡ Blazina: Prag. Viertel. der prakt. Heilk., 1844; and St. Thomas's Hospital Museum.

Luxation of both radius and ulna,	1. backwards,	*a.* complete. *b.* incomplete.
	2. forwards,	*a.* with fracture of the olecranon, *b.* without fracture of the olecranon.
	3. outwards,	*a.* complete. *b.* incomplete.
	4. inwards,	*a.* complete. *b.* incomplete.
	5. Simultaneous luxation of both bones in opposite directions.	
Luxation of the ulna,	backwards.	
Luxation of the radius,	1. backwards, 2. forwards. 3. outwards. 4. incomplete.	

We have not space in the present review to do more than direct attention to those varieties which are least common. Great praise, however, is due to Gurlt for the care and accuracy with which he has arranged a vast amount of information, collected from different sources.

Complete Luxation of both Bones of the Fore-arm forward without Fracture.—The coronoid process and the head of the radius were found upon the anterior surface of the humerus, above the articular surface; the extremity of the olecranon rested upon the lower border of the posterior supra-trochlea fossa, where it had made for itself a special cavity.*

Two cases of incomplete luxation backwards are mentioned by Gurlt; one related by Gély,† a second by Girdlestone,‡ who describes a specimen taken from the dissecting-room of St. Bartholomew's Hospital, by Mr. Holmes Coote. These appear to be the only two yet published, and they agree singularly in particulars; in both, the coronoid process was placed on the middle or the under part of the trochlea, and the head of the radius was close under the extremity of the humerus: new articulations had been formed around.

The Museum of the Royal College of Surgeons of England contains a specimen (No. 3278), in which the radius is dislocated forwards, and the ulna outwards instead of backwards, as is the more common.

Luxations of the head of the radius forwards or backwards are very common. One case of luxation outwards has been described by Nelaton:§ the annular ligament was of course ruptured.

Fractures about the elbow joint are arranged as follows:

"1. Fracture of the humerus, just above the condyles, or through them into the joint.

"2. Fracture of the external condyle.

* Anat. Mus. of the University of Berlin, No. 2253.

† Journal de Chirurgie, par Malgaigne, May, 1844, p. 139.

‡ London Medical Gazette, vol. xi. p. 138, 1850.

§ Ein von Nelaton beobachteter Sectionsbefund, in der Concours-These, von Huguier, 1842.

3. Fracture of the trochlea or internal condyle.
4. Fracture of both bones of the fore-arm close to the elbow joint.
5. Fracture of the olecranon.
"6. Fracture of the coronoid process.
7. Comminuted fractures."

Fractures of the olecranon usually unite by ligament, but occasionally also by bone, as proved by a specimen in the Musée Dupuytren (No. 105). The head of the radius and the ulna are here dislocated forwards. The fracture, which was from above downwards, from before backwards, and from within outwards, had divided the ulna into two fragments. Instances are upon record in which the olecranon has not united, or the fracture has been but partially repaired. In such cases, the power of extending the fore-arm is much impaired, or even lost, and the limb is weak and comparatively useless.

Diseases of the Hand and Wrist.

In this important part of the upper extremity, where many small bones contribute to form the articulation, we find inflammatory affections passing into ulceration; chronic inflammation, which, however, more commonly attacks the carpo-metacarpal, or the metacarpo-phalangeal articulations, than the wrist. Gouty deposits are very common; cancer is rare. An instance of synostosis between the ossa semilunare and cuneiform is preserved in the Anatomical Museum of Berlin (No. 7363). A case of club-hand, from congenital deficiency of the radius, is described by Roger and Honel.*

Congenital luxation of the carpus has been noticed on the volar surface of the fore-arm, by Cruveilhier,† and both on the volar and dorsal surfaces, by R. W. Smith.‡

Of traumatic luxation there is no specimen, nor any well-authenticated case yet published.

When we consider how numerous and how complicated are the joints composing the wrist, how constantly, to the last hour of existence, they are in movement, to what violence they are exposed, we may wonder that disease is not of more common occurrence. Although Gurlt states that inflammation of the carpal bones is very frequent, yet he confesses to the lack of anatomical material whence to draw his description, a statement we should hardly expect, were morbid changes as abundant as he infers. As far as our observations extend inflammatory disease of the wrist occurs chiefly (1) in persons of strumous habit, being of chronic character, and curable with difficulty, from the great tendency to relapse; (2) in persons subject to rheumatism or gout. We have seen it as a consequence of gonorrhœa, in a case where it produced much thickening of surrounding tissues, and great impairment of motion.

Redfern§ has accurately described the morbid appearances resulting from inflammation of the hand, the carpal, and the metacarpal joints in a man, aged 19. All the tissues around the carpus were infiltrated by a gelatinous fluid; fistulous passages extended in various directions; the

* L'Union Médicale, 1851, No. 140. † Anat. Pathol., Livrais 9.
‡ Fractures in the Vicinity of Joints. p. 121.
§ Abnormal Nutrition in the Human Artic Cartilages, p. 18.

bones were so firmly united, that they broke when forcibly separated, fragments remaining adherent to the remains of the articular cartilages. Where there was no cartilage, the bones were rough. The opposed surfaces of the radius and ulna were in the same condition; but the inferior extremity of the latter still retained its articular cartilage. The bones were tolerably firm. In the joint between the rows of carpal bones there was scarcely any cartilage left. Those composing the upper row separated easily one from another, also those of the lower, and between them was a white fibrinous mass. The carpal end of the fifth os metacarpi and the lower part of the os cuneiforme, were wanting, a mass of granulation occupying the space. The Museum of St. Bartholomew's Hospital contains several specimens in illustration of this subject. The carpal bones (Ser. I., No. 40) may be nearly destroyed by deep ulceration, osteophytes may form around, the radius may become necrosed at its lower extremity (Ser. II., No. 13), and the surrounding tissues hardened; but further examination of pathological collections proves, that in the wrist there is a power of repair sufficiently great to induce the surgeon to delay as long as possible any operation by which the patient would be deprived of a part so useful as the hand and fore-arm. In the Anatomical Museum of the University of Berlin, there is a preparation (No. 6853) of complete anchylosis of the hand and carpus. The os metacarpi pollicis alone is movable. A similar preparation exists in the Museum of the Royal College of Surgeons of England (No. 3350). The same fact is illustrated by specimens in the Museum of Guy's Hospital, of the Surgical Klinik of the University of Berlin, and by a case recorded by Sandifort.*

Chronic Inflammation occurs under various forms in the wrist. The Museum of the Royal College of Surgeons of England contains two specimens (Nos. 627 and 627 A), in which bony processes have been developed around the inferior articular surfaces of the radius and ulna; and St. Bartholomew's Museum possesses one in which the inferior articular surfaces between the radius and ulna are hard, polished, and grooved, and new bone has been formed around (Sub-ser. II. B, No 37). But the most common disease is that dependent upon gout, which more especially attacks the smaller joints, but yet occasionally leaves its deposits in both carpus and metacarpus. The concretion has been found also both in the spongy texture of the bones and along the course of the tendons.

The only case of cancer affecting the wrist, recorded by the author, is that related by Mr. R. Adams.†

Congenital Luxations at the wrist constitute one of the varieties of club-hand. There is usually no trace of the carpal articulating extremity of the radius, but a new joint is formed, either upon the dorsal or volar surface, with which the atrophied carpal bones articulate, at an angle. The ulna, generally longer than the radius, preserves more of its normal form. Cruveilhier‡ has recorded a case of luxation upon the volar surface of the fore-arm, in an adult female, the hand being united to the radius at a right angle in flexion; the radius was short and deformed, but a strong process connected it with a cavity in the ulna. Mr. Smith, in his excel-

* Mus. Anat., vol. iv. Tab. 156.; and vol. iii. p. 246.
† Dublin Journal of Medical Science, vol. xvii. p. 490.
‡ Anat. Path., avec Planches, Livrais 9, Pl. 2.

lent work, so often quoted, gives an instance of double congenital luxation, one on the volar, the other on the dorsal surface,* with an accurate account of the condition of the carpal bones, and the relations of the surrounding tendons; but of traumatic luxation, Gurlt, in common with other authors who write either from personal experience, or with a careful analysis of the evidence which they bring forward, can furnish no example. The "dislocated wrist," so commonly talked of, not twenty years hence, as demanding sudden and violent extension, proves either a simple sprain, or fracture of the lower extremity of the radius. The twisting of the hand from cicatrices, of which Cruveilhier speaks; the sub-luxation among particular classes of workpeople, mentioned by Dupuytren, cannot properly be included under this head. The only instance of which we have heard (the particulars are not published), where the possibility of such an accident can be admitted—and we must confess that the evidence is doubtful—occurred to one of the workmen employed in raising a large and heavy piece of stone, during some works in St. Bartholomew's Hospital. The stone suddenly slipped from the machine a short distance, against the hands extended and pressing upon the piece, which was of large size and enormous weight. It was affirmed by the gentleman who saw the case, and immediately did what was necessary, that he felt the extremity of the carpus lying upon the dorsal surface of the fore-arm: reduction was easy, and the displacement did not recur. In most cases of doubtful injury to the wrist, the radius is broken close to the joint, the lower fragment being drawn backwards. When the accident is properly treated, union takes place readily by bone; but when the surgeon, labouring under the mistake that the bones are merely dislocated, subjects his patient to frequent attempts at reduction, the surrounding tissues inflame, the tendons become agglutinated, and the movements of the hand and fingers are permanently impaired. In Ireland, this is called "Colles' fracture."

The metacarpal and phalangeal articulations exhibit, in small, the same changes which go on in the larger articulations of the wrist or elbow. The synovial membrane may suffer from either acute or chronic inflammation; gouty deposits are especially common; necrosis of the phalanges ensues, both as a consequence of acute inflammation, as whitlow, or in strumous subjects, where there has been no active disease. In all, we would impress upon the surgeon the fact that the parts possess great powers of repair, and that amputation should not be hastily recommended.

Upon the development of enchondroma, which is particularly common in the long bones of the hand, we must refer to works upon tumours of bone; no remedy exists save the removal of the limb.

Holmes Coote.

(*To be continued.*)

* Fractures in the Vicinity of Joints, p. 121.

Review VI.

1. *De la Mort et de ses Caractères.* Par le Dr. Josat.—*Paris,* 1854. 8vo, pp. 375.
2. *Du Signe Certain de la Mort.* Par Michel-Hyacinthe Deschamps. —*Paris,* 1854. 8vo, pp. 240.
3. *Traités des Signes de la Mort.* Par E. Bouchut.—*Paris,* 1849. 8vo, pp. 407.
4. *The Medical Aspects of Death.* By James Bower Harrison.—*London,* 1852. Small 8vo, pp. 165.

The subject of the treatises above named has, it is evident, attracted much greater attention on the Continent than in our islands. We have here three elaborate works recently published in France, each of which has its independently distinctive features; while of the solitary essay that has emanated from the British press, it cannot be said that its author has done more than reproduce, in a popular form, what has been already written by others. In an admirable article which appeared in the 'Cyclopædia of Anatomy and Physiology,' twenty years ago, the author, Dr. Symonds, quoted from two British, and at least a dozen foreign, medico-legal writers. In the bibliography appended to one of the treatises now before us, we count only four English names among the number of forty-seven authors who have treated of the signs of death. It cannot, however, be denied, that much of the material so abundantly produced has consisted of a repetition of the same facts, and, often enough, of the same falsehoods; for in some of these authors there is apparent a credulity equal to every story of live-death that ever was invented by the most incorrigible horror-monger. The object, however, of the essays now under consideration, has not been to perpetuate the incredible fabrications of excited imaginations, but philosophically to recognise undeniable accidents, and then scientifically to look for the means of their prevention.

We may, in the first place, clear our way by disengaging from our subsequent remarks Mr. Harrison's brochure. We do this in all courtesy and respect for its author, who will be ready to admit that his book was not written to assist professional research, but rather to present, in a small compass and in a readable shape, a topic, painful, but useful to be thought upon by non-professional readers: nor with the intention of unfolding any new scientific views, or of suggesting any improved practice as regards interment, but partly to satisfy his own "natural curiosity about death," and partly "to fill up the intervals left by more arduous professional studies." We concede that he has, in thus addressing the public, manifested judgment and discretion in the execution of his object. The topics discussed by Mr. Harrison are premature burials, signs of death, putrefaction, modes of death, &c. The scope of the work required the illustration of these several topics by sundry quotations from the wide field of general literature—e.g., among others, 'The History of the Tragedy at the Black Hole in Calcutta;' 'The Account of the Opening of the

Coffin containing the Remains of Charles I.;' 'Conversations of Barry O'Meara with Napoleon, upon the Easiest Mode of Dying;' 'The Narrative of the Death of Dr. Arnold, of Rugby;' *cum multis aliis.* The whole constituting, as we have said, a readable, popular exposition of a very solemn subject.

In making our readers acquainted with the contents of the three French treatises, we shall pass over the enumeration of the signs of death to be found in each, as these may be met with in every systematic work upon medical jurisprudence; confining our attention to those particular signs which are dwelt upon by each author as having the closest connexion with the great practical objects of all these investigations,—viz., the recognition of the state of apparent death, and the prevention of premature interment.

The treatise of M. Josat is, he informs his readers, the work of upwards of ten years' patient and conscientious research. His labours originated in the impression made upon the public mind in France by the frequent recitals of premature interments recorded in the newspapers. The government of Louis Philippe, in order to ascertain the truth or falsehood of these rumours, and with a view to their future prevention, instituted an official investigation, preparatory to a revision of the laws regarding interment. The burial of the dead had been already placed under wiser regulations in Germany, and to obtain a more intimate acquaintance with these the author was entrusted with a mission to the German States. The results of his inquiries are embodied in this work, of which M. Rayer, in his report thereon to the Academy of Sciences, observed,—"M. Josat's is not a work merely made up from other books, but it is a long and important work, which, for its own merits, deserves an attentive examination."

M. Josat, besides treating of the ordinarily received signs of death, which he seriatim examines and criticises, gives a full exposition of the legislative enactments regarding interment at present in force in France and Germany. The author concludes from his researches that there is no single sign of death which can be regarded as satisfactory, except putrefaction. Having arrived at this conclusion, Dr. Josat enters upon the consideration of the subject of apparent death, and the means of preventing premature interment. These we shall presently more fully bring under the notice of our readers.

We proceed in this place to compare the views of Dr. Josat with those of M. Deschamps, who also examines and rejects all the signs of death, except that of green discoloration of the abdomen, which he asserts is an invariable and infallible proof that life is totally extinct. Dr. Josat sees the early evidences of decomposition, at uncertain periods after death, in the changes of colour presented by the integuments of the abdomen about the inguinal regions, which he says become of a dull, white, whitish-grey, blue, green, or black colour, and spreading over the abdomen as the tints deepen; and, at the same time, that a peculiar cadaveric odour becomes perceptible. The occurrence of the phenomena is much influenced by the nature of the disease which caused death, as well as other internal conditions of the corpse, and external circumstances by which it is surrounded. It is not, however,

pretended by Dr. Josat that these postmortem changes should be waited for in every case, as in the majority of instances of death no room for doubt exists. The views of M. Deschamps, although seemingly similar to those of Dr. Josat, are not precisely the same. M. Deschamps asserts, with the fullest confidence, that the single phenomenon of green discoloration of the abdomen is a sufficiently certain sign of death. It is, therefore, not required to wait for further putrefaction, as this green discoloration, this veritable stamp of death, precedes other putrefactive changes in the structure of the abdominal parietes, and is unattended with fetid odour. The same discoloration perceived on the limbs, the author says, is not an equally sure sign of death, since these may, at any time, during life, be removed without destroying the body. The author points out that the period at which this sign may make itself seen will vary. Generally, it will not be present until the warmth of the body has passed away. It coincides most frequently with the occurrence of cadaveric rigidity. It is absent, also, so long as the muscles retain their susceptibility to galvanic stimulus. We must here remark, that in reading Dr. Josat's work we have met with an exception to this rule, in the case of a body the arms of which were susceptible to galvanic excitation although perfectly cold, and had passed through the state of cadaveric rigidity, having returned to that of cadaveric pliancy, and being even partially decomposed. M. Deschamps remarks, that the appearance of the green discoloration may be retarded from twelve to fifteen days by an exposure to a freezing temperature, while on the occurrence of a thaw putrefaction will rapidly supervene. A corpse exposed during one day, in a chamber at a temperature of from 65° to 75° Fahr., will often exhibit this sign of death by the evening. At higher temperatures,—e.g., from 130° to 140° Fahr.—the corpse is dried, and putrefaction is arrested. In death from acute inflammations and effusions, in puerperal fever, in dropsies, in infancy, the green patch occurs quickly. It appears, also, earlier in females than in males; in the young than in the old. It is stated by M. Deschamps that the fœtus that has died in ütero does not present a green, but a red discoloration of the abdominal integuments. The appearance of this sign is retarded by submersion in water, but it still appears first on the abdomen. A range of from a few hours to twenty days has been noticed by M. Deschamps with regard to the date of its appearance;—the average would seem to be about three days. Its occurrence is accelerated by all those conditions which preserve the warmth of the body, as well as those which accelerate or promote putrefaction; it is retarded by very low or very high temperatures, and by the use of antiseptics. As the phenomenon in question is caused by air, warmth, and moisture, so its appearance can be accelerated by the exposure of the corpse to these conditions in a chamber; its advent can there be watched, rendering it unnecessary to expose survivors to the risk of the noxious exhalations of putrefaction. The seat of the discoloration is subepidermic. Under the microscope, it is seen to be composed of green granules, resembling the minute structure of vegetable mould. No other discolorations caused by disease, the author asserts, can be mistaken for this, his infallible sign of death.

We may here insert the statistics on the probable duration of apparent

death, which M. Josat, casting aside the marvellous improbabilities that have been written, gives from his own observation in 162 instances. The state of apparent death lasted in—

7	from	36 to 42	hours.
20	„	20 to 36	„
47	„	15 to 20	„
58	„	8 to 15	„
30	„	2 to 8	„

The order of frequency of diseases in which these occurred was as follows: asphyxia, including still-births; syncope; hysteria; apoplexy; narcotism; concussion of the brain. The apparent death lasted longest in hysteria, and shortest in concussion.

We have, in the next place, to direct attention to the leading characteristic of M. Bouchut's essay, *which, allowing all other signs of death to be fallacious, claims the merit of certainty for the absence of the sounds* of the heart when prolonged beyond one or two minutes. This opinion is founded upon the physiological axiom which regards the heart as the *primum vivens, ultimum moriens,* and has been strengthened by the author's observations and researches in cases of syncope, lethargy, poisoning by narcotics, &c. Notwithstanding, however, that M. Bouchut's researches have been extensive, and scientifically conducted, his conclusion is not so trustworthy as he himself, and the Commission of the Academy of Sciences, were willing to believe. M. Josat has recorded several instances wherein newly-born children have been most carefully examined during several minutes, without the detection of the slightest cardial sound or movement, and yet these have rallied and lived. M. Depaul has collected ten similar instances. M. Brachet has recorded* an instance of a man in whom neither sound nor movement of the heart could be heard for eight minutes, and who nevertheless survived. Another adult case is mentioned by Dr. Josat, as having been witnessed by M. Girbal, of Montpellier.

The results of experiments that have been instituted by physiologists, to ascertain the actions of various poisons, have also tended to conclusions adverse to those of M. Bouchut, and to the axiom with which he starts. The heart has been seen beating in the exposed thorax of an animal several minutes after life has been extinct, and even after the brain and spinal cord have been destroyed. Sir B. Brodie and others have described children born without hearts. The circulation is maintained at one period of human life without the aid of the heart. It is, besides, quite consistent with the facts observed in hysterical and other morbid conditions of the nervous system, that the action of the heart, like that of other muscles, should be so extremely feeble as not to be cognisable by any sound or impulse, and yet it may have sufficient movement slowly to move the blood through a system whose every function and endowment is suspended, and all but annihilated. In cases of catalepsy, and of authentic instances of apparent death, the respiratory muscles have not been seen to move, yet inspiration and expiration, however slowly and

* Bulletin Thérap. Méd., tom. xxvii. p. 371.

imperceptibly, must have taken place. So may it be with the heart. If so, then M. Bouchut's test loses its character of infallibility.

Having in the preceding observations dwelt upon some of the most prominent features of these works, we conclude with a brief abstract of the results of Dr. Josat's investigation on the modes of preventing premature interment, which he has found in force in the German states, as well of the provisions existing in France at the present time, with the author's proposed reforms.

The earliest movements in the direction of means for the prevention of premature interments originated with Winslow, in France, to whom followed other known French medical writers upon signs of death. It was Madame Necker, however, who embodied their suggestions in a practicable form as submitted to the National Assembly in 1792 by Count Berchshold. In the ninth year of the former French Republic a project was entertained for the erection of six "*temples funeraires*" in Paris, but came to no good end, as attendant evils preponderated. To Germany belongs the credit of having executed these designs in such wise that they should not prove the positive sources of more danger to the living than could be counterbalanced by the occasional preservation of an individual from the risk of premature interment. A belief in this same risk having been prodigiously diminished since the establishment of institutions for the reception of cases where doubt of the reality of death has existed, Hufeland, in Weimar, devised the plan that Frankfort-on-the-Maine incorporated with its reform in sepulture and establishment of extramural cemeteries in 1823.

Hufeland's plans have subsequently been adopted and carried out in many other German states. The arrangements described by Dr. Josat, as witnessed at Frankfort, represent those adopted in other parts of Germany. They are deserving of attention, both from their author's celebrity and from their intrinsic ingenuity.

The description of these establishments in Dr. Josat's work is accompanied by engravings, which facilitate the comprehension of their structural details. We can only lay before our readers their broad features. Attached to each of the public cemeteries, which are constructed on the heights surrounding the city, is on one side of a quadrangle, a series of buildings, comprising chapel, directors', and servants' apartments. On the other side is a *salle de veille*, or *watch-room*. In the whole length of this chamber is a series of glazed sashes, each corresponding to a separate cell, and above each window a loud bell, termed the *alarm-bell*. This ominous alarm-bell is struck by a hammer which communicates with the interior of the cell by a simple mechanism in contact with the body placed therein as one of reputed apparent death—and only under these circumstances.

Each of these *mortuary cells* is so built so to insure ventilation and temperature at the will of the director. In each is placed a stand, upon which is laid the bier conveying the body. The limbs of the body are so arranged and brought into connexion with the mechanism belonging to the alarm-bell, that it is impossible that the slightest movement can take place in the body without an alarm being given, and an attendant summoned. A medical officer, with competently educated assistants, are

constantly upon the premises, furnished with all the means and appliances for fostering any indication of returning life.

A body deposited in these institutions is detained until decomposition has commenced its operations, when the friends are informed thereof, and interment takes place in the cemetery in as quiet a manner as possible.

Dr. Josat detects several objections to various parts of the arrangements he has described. The most serious of these is that in certain cases of syncope and hysterical trance the first indications of life are to be traced in a slight change of complexion, or a scarcely perceptible movement of the eyelids, of the thoracic muscles, or of the heart, to all of which the apparatus would be unavailable.

In Mayence, Dr. Josat states that all bodies are exposed, until decomposition takes place, in large chambers constructed for the purpose. There bodies are often left so long that these places become charnel-houses, distributing far greater danger than the risk of premature burial. In Berlin also, and many other Prussian towns, bodies are exposed until decomposition occurs—but the bodies are here carefully watched and attended with the greatest care, by skilful officers, and interment takes place on the earliest appearance of putrefaction. We learn from a recent return that in Berlin, out of a population of 430,000, only 48 corpses were brought to dead-houses. Not a single instance has occurred of resuscitation.

In Austria, bodies are received into dead-houses on the request of the friends. In any case interment does not take place until sixty-two hours after death, as certified by a medical man.

In France, twenty-four hours is the term fixed by law for interment. Restoration from apparent death has frequently taken place later than twenty-four hours; consequently the law favours premature interment, and offers opportunities for the concealment of crime. An official medical attestation of death is required within the twenty-four hours, but it is obvious that this offers no protection or assistance to cases of apparent death lasting shorter periods, before the arrival of the attesting medical officer.

As a remedy for these dangers, the author proposes that no corpse should be interred until it presents signs of decomposition, which, he observes, will usually be apparent within seventy-two hours; that chambers should be attached to every cemetery for watching every reputed death, without regard to rank or condition of the individual; that a circular arrangement of cells constructed for the purpose would afford sufficient accommodation, according to the population or average mortality of each place. The author describes minutely the arrangements he suggests for the recognition of signs of life in the bodies thus exposed; these are modifications or improvements upon those of Frankfort.

As a sanitary measure, the separation of the dead from the living, especially from among the crowded poor, would be, apart from the not less important point of the verification of death, an incalculable benefit to France—to Paris: also to England—to London.

The histories of premature interments are in all cases to be received with hesitation. During epidemic visitations the most alarming accounts are greedily believed and magnified, all the while being totally devoid of truth. It is, however, not to be denied that such things have happened,

and may therefore happen again. It has been urged that, even admitting the possibility of a person being buried alive, it is not possible that he could wake to consciousness in so small a quantity of air as is enclosed in a coffin under ground; that there being little more than enough to fill the chest, it would become so quickly vitiated that real death by asphyxia would be almost instantaneous. The statement is plausible, but we fear is not enough to divest of its unutterable horror the very thought of such a predicament. The truth is, that facts are stubborn things, and will not do obeisance to all theories. It behoves us in this matter to learn another lesson from our neighbours, and to take measures to prevent the occurrence of catastrophes too fearfully horrible to contemplate in thought—too dreadful for the most vivid or the most morbid imagination to realize. Science can hold out no token by which to recognise the certainty of death. Sanitary police, at least in England, is indifferent about the risk of a few burials alive, and thinks it superfluous to prevent their occurrence.

W. B. Kesteven.

REVIEW VII.

1. *Cure and Prevention of Scarlet Fever.* By SAMUEL HAHNEMANN. (Lesser Writings of the Author, collected and translated by R. E. DUDGEON, M.D.)
2. *Travàux Thèrapeutiques sur la Belladone.* Publiée par A. L. J. BAYLE. (Tome Seconde de 'Bibliothèque de Thérapeutique.')—*Paris*, 1830.
3. *Homœopathy: its Tenets and Tendencies.* By Professor SIMPSON.—*Edinburgh*, 1853.
4. *Homœopathy fairly represented.* By Professor HENDERSON.—*Edinburgh*, 1853.

SINCE the immortal discovery of Jenner, whereby one of the most frightful and most fatal diseases from which the human race has ever suffered was deprived alike of its terrors and its victims, the cultivators of medicine have been justly animated by the hope that their science might be caused to yield other services of a kindred nature to mankind.* Nor has there been any want of real and earnest activity in a work which, since the introduction of vaccination, all must have had more or less at heart. For whether or not we concede to belladonna the prophylactic virtues in scarlatina which not a few have claimed for it, we are at all events called upon to acknowledge, that from very many the subject has, at various times during the five-and-fifty years it has been under discussion, received all that attention and patient investigation which every right-thinking man will readily and heartily admit to be its due.† That a disbelief in

* "I believe," says Dr. Simpson, "medicine will yet most probably discover prophylactic measures against scarlet-fever, measles, &c."—Homœopathy, p. 230, note at foot of page.

† We may remind our readers that very many other prophylactics have been recommended and actually employed in scarlatina besides the exhibition of belladonna. In regard to such we find Joseph Frank writing, "Ad scarlatinam præpediendam commendata fuere: errhina et collutoria ex ammonia cum sufficiente quantitate aquæ; acida mineralia diluta tum interne, tum externe sub forma gargarismatis: subfumigia vel ope acidi muriatici sive simplicis sive oxygenati, vel ope acidi nitrici; minimæ doses succi inspissati herbæ atropæ belladonnæ; et ipsa

the alleged power of belladonna should have taken possession of the mind of the profession generally, and more particularly in this country, was scarcely to be wondered at, when we consider the quarter from which the recommendation of its vaunted virtues proceeded, and the manner in which the test of its efficacy was required to be determined. But though apologizing for the feeling at first entertained by the bulk of medical men in regard to the announcement of the prophylactic action of belladonna, we are not to be held as thereby approving it, far less defending the course of procedure which it in some cases engendered; for, on the contrary, when regard is had to the frequency as well as to the extremely fatal nature of many epidemics of scarlet fever, whose ravages it was upheld both to mitigate and repress, we do feel that the mere circumstance of Hahnemann being its originator and strongest advocate formed no excuse for belladonna being either neglected or passed by. Some there were who entered at once upon the examination and investigation; and during the lengthened period that has since elapsed, abundant opportunities have been seized and turned to the best account.

But while we readily allow the same privilege to Hahnemann and his followers as we claim for the disciples of our own School, in so far as the propriety of investigating the peculiar virtue claimed for belladonna by the former was incumbent upon both, we at the same time do conscientiously believe, that had it owed its suggestion and enforcement to such a physician as Laennec, or even to Bayle,* the question of the prophylactic action of belladonna would have long ere this been settled, in the one way or in the other. Once propounded, the claim advanced would have been rigidly examined, and not accepted as correct by some upon what we shall presently show to have been most insufficient grounds, nor rejected in several instances, as we believe, upon grounds certainly not more reliable. For ourselves, we are clearly of opinion that the time and occasion have now arrived when the question of the prophylaxis of belladonna can readily and satisfactorily be answered—and, as we think, in the negative. But, desirous as we are of doing our opponents—we have now declared our own view—every justice, and the subject itself being full of interest, we shall devote this article to a reconsideration of the whole matter.

Although the discovery of the supposed prophylactic action of bella-

scarlatinæ insertio, de quarum autem rerum effectibus, cum non quivis homo necessario scarlatinæ subjici debeat, arduum est judicare."—Praxeos Medicæ Universæ Præcepta, vol. ii. par. i. p. 221. Leipzig, 1815.

"Si l'action de la belladone est encore douteuse, malgré le grand nombre de ses partisans, celle des autres préservatifs est encore bien plus hypothétique. Ainsi on a prôné une combinaison de soufre doré et de calomel. La dose, pour les enfants de deux à quatre ans, est d'un sixième ou d'un huitième de grain de calomel uni à autant de soufre doré d'antimoine, et mêlé à un peu de sucre ou de magnésie; on répète cette dose trois où quatre fois par jour. Cette méthode a été conseillée par un médicin hollandais (E. J. Thomassen, à Thuessink), qui affirme que dans toutes les familles où l'on fit l'usage du préservatif, la scarlatine ne sévit pas. Il cite l'observation d'un enfant qui sous son influence n'eut ni mal de gorge ni éruption, mais la desquamation consécutive."—Traité Clinique et Pratique des Maladies des Enfants, par MM. Barthez et Rilliet, deuxième édition, tom. iii. p. 208.

* Bayle manifestly gave it his support, but to a certain extent, and in a certain sense, the theory whose associated facts Bayle has done much service in recording, was tarnished in its propounder.

donna in scarlatina has been attributed to Castelliz, of Vienna,* there appears little doubt that the idea originally occurred to the mind of Hahnemann, and no doubt that by him the subject was first introduced to the notice of the profession. The former happened when he was resident at Königslutter, in 1799. Two years thereafter he published a pamphlet, entitled 'Heilung und Verhütung des Scharlachfiebers,'† from the translation of which, in Dr. Dudgeon's edition of the lesser works of Hahnemann, we extract his own account of the manner in which his discovery was made.

"The mother of a large family, at the commencement of July, 1799,‡ when the scarlet fever was most prevalent and fatal, had got a new counterpane made up by a sempstress, who (without the knowledge of the former) had in her small chamber a boy just recovering of scarlet fever. The first-mentioned woman on receiving it examined it, and smelt it, in order to ascertain whether it might not have a bad smell that would make it necessary to hang it in the open air; but as she could detect nothing of the sort, she laid it beside her on the pillow of the sofa, on which some hours later she lay down for her afternoon's nap. She had unconsciously, in this way only (for the family had no other near or remote connexion with scarlatina patients), imbibed this miasm. A week subsequently she suddenly fell ill of a bad quinsy, with the characteristic shooting pains in the throat, which could only be subdued after four days of threatening symptoms. Several days thereafter her daughter, ten years of age, infected most probably by the morbific exhalations of the mother, or by the emanations from the counterpane, was attacked in the evening by severe pressive pain in the abdomen, with biting itching on the body and head, and rigour over the head and arms, and with paralytic stiffness of the joints. She slept very restlessly during the night, with frightful dreams, and perspiration all over the body excepting the head. I found her in the morning with pressive headache, dimness of vision, slimy tongue, some ptyalism, the submaxillary glands hard, swollen, painful to the touch, shooting pains in the throat on swallowing and at other times. She had not the slightest thirst, her pulse was quick and small, breathing hurried and anxious; though she was very pale she felt hot to the touch, yet complained of horripilation over the face and hairy scalp; she sat leaning somewhat forwards, in order to avoid the shooting in the abdomen, which she felt most acutely when stretching or bending back the body; she complained of a paralytic stiffness of the limbs with an air of the most dejected pusillanimity, and shunned all conversation. 'She felt,' she said, 'as if she could only speak in a whisper.' Her look was dull and yet staring, the eyelids inordinately wide open, the face pale, features sunk.

"Now I knew only too well that the ordinary favourite remedies, as in many other cases, so also in scarlatina, in the most favourable cases leave everything unchanged; and, therefore, I resolved in this case of scarlet fever just in the act of breaking out, not to act as usual in reference to individual symptoms, but if possible (in accordance with my new synthetical principle) to obtain a remedy whose peculiar mode of action was calculated to produce in the healthy body most of the morbid symptoms which I observed *combined* in this disease. My memory and my written collection of the peculiar effects of some medicines, furnished me with no remedy so capable of producing a counterpart of the symptoms here present as *belladonna*.

"It alone could fulfil most of the indications of this disease, seeing that in its primary action it has, according to my observations, a tendency to excite even in

* See Lectures on Materia Medica and Therapeutics, by G. G. Sigmond, M.D., Lecture xiii. Lancet, Vol. ii. 1836, 37.

† Originally published at Gotha, in 1801.

‡ The 14th of May, 1796, was, as Dr. Watson happily terms it, "the birthday of vaccination;" it is not unlikely that during the period from May, 1796, to July, 1799, the mind of Hahnemann had been strongly directed to the subject of the prevention of contagious diseases.

healthy persons great dejected pusillanimity, dull staring (stupid) look, with inordinately opened eyelids, obscuration of vision, coldness and paleness of the face, want of thirst, excessively small rapid pulse, paralytic immobility of the limbs, obstructed swallowing, with shooting pains in the parotid gland, pressive headache, constrictive pains in the abdomen, which become intolerable in any other posture of the body besides bending forwards, rigour and heat of certain parts to the exclusion of others—e. g., of the head alone, of the arms alone, &c. If, thought I, this was a case of approaching scarlet fever, as I considered was most probable, the subsequent effects peculiar to this plant,—its power to produce synochus, with erysipelatous spots on the skin, sopor, swollen hot face, &c.,—could not fail to be extremely appropriate to the symptoms of fully developed scarlatina.

"I, therefore, gave this girl, ten years of age, who was already affected by the first symptoms of scarlet fever, a dose of this medicine ($\frac{1}{432000}$th part of a grain of the extract, (which, according to my subsequent experience, is rather too large a dose.) She remained quietly seated all day, without lying down; the heat of her body became but little observable; she drank but little; none of her other symptoms increased that day, and no new ones occurred. She slept pretty quietly during the night, and the following morning, twenty hours after taking the medicine, most of the symptoms had disappeared without any crisis; the sore throat alone persisted, but with diminished severity, until evening, when it too went off. The following day she was lively, eat and played again, and complained of nothing. I now gave her another dose, and she remained perfectly well, whilst two other children of the family fell ill of bad scarlet fever without my knowledge, whom I could only treat according to my general plan detailed above. I gave my convalescent a smaller dose of belladonna every three or four days, and she remained in perfect health. I now earnestly desired to be able, if possible, to preserve the other five children of the family perfectly free from infection. Their removal was impossible, and would have been too late. I reasoned thus: a remedy that is capable of quickly checking a disease in its onset, must be its best preventive; and the following occurrence strengthened me in the correctness of this conclusion. Some weeks previously, three children of another family lay ill of a very bad scarlet fever; the eldest daughter alone, who, up to that period, had been taking belladonna internally for an external affection on the joints of her fingers, to my great astonishment, did not catch the fever, although during the prevalence of other epidemics she had always been the first to take them.

"This circumstance completely confirmed my idea. I now hesitated not to administer to the other five children of this numerous family this divine remedy as a preservative, in very small doses, and as the peculiar action of this plant does not last above three days, I repeated the dose every seventy-two hours, and they all remained perfectly well, without the slightest symptoms throughout the whole course of the epidemic, and amid the most virulent scarlatina emanations from their sisters who lay ill with the disease. In the meantime I was called to attend another family, where the eldest son was ill of scarlet fever. I found him in the height of the fever, and with the eruption on the chest and arms. He was seriously ill, and the time was consequently past to give him the specific prophylactic treatment. But I wished to keep the other three children free from this malignant disease. One of them was nine months, another two years, and the third four years of age. The parents did what I ordered, gave each of the children the requisite quantity of belladonna every three days, and had the happiness to preserve these three children free from the pestilential disease, free from all its symptoms, although they had unrestricted intercourse with their sick brother. And a number of other opportunities presented themselves to me where this specific remedy never failed." (p. 434.)

Such is Hahnemann's account of the mode in which the efficacy of belladonna was first suggested to his own mind. We shall anon revert to the passage we have quoted at such length; meantime, let it be

observed, that over and above the prophylactic virtue in scarlatina which Hahnemann claims for belladonna, he also asserts its potency as a specific remedy in the disease itself, modifying its symptoms, removing its "after sufferings," or consequences, "often worse than the disease itself;" and capable, too, of suppressing the fever "in its first germs," when its invasion has already occurred. Further, that, *so far as the prophylaxis of belladonna* is concerned, Hahnemann makes no restriction of the cases of true scarlatina in which the drug may either be inadmissible, or may, in his own experience, have proved useless.* On the contrary, we are led to suppose that, in his experience, no such cases occurred. And this view of his own opinion is rendered more than probably correct, when we find him speaking in his greater work thus:†—"Et qu'en prenant une dose de belladone aussi faible que possible, on se garantit de la fièvre scarlatine."

One of the earliest notices, if not the first mention of the alleged virtues of belladonna, which appeared after the publication of Hahnemann's own pamphlet, and corroborative of his views, was in Hufeland's Journal, for May, 1812, from the pen of Dr. Schenck, having reference to an epidemic which occurred in the department of Hilchenbach, in the grand duchy of Berg. It and the other testimonies which follow, both in favour of and against belladonna, are detailed, for the most part, with much precision in the learned work of Bayle.‡

In 1812, when Schenck witnessed the effects of belladonna at Hilchenbach, the epidemic had, before his arrival, lasted for three weeks. Eight persons had already died, two of whom were previously healthy and robust young men, and two young women in like condition. Twenty-two were then affected; almost all were children, or young persons below the age of twenty. Of 525 persons who used the belladonna, 522 were unattacked by the disease. The three persons who suffered were a mother and her two children, who were, it is said, peculiarly exposed to the contagion, and had only taken the drug four times. The manner of making and administering the preparation of belladonna adopted by Schenck, to whom it was suggested by Hahnemann himself, is thus recorded, and as this is important, we shall quote the French of Bayle:

"M. Hahnemann eut la bonté de me faire parvenir trois grains d'extrait de belladone qu'il avait préparés lui-même, attendu qu'on le confectionne pas dans toutes les pharmacies avec assez de soin pour qu'on puisse compter sur son effets. Il m'envoya en même temps l'instruction suivante: On triture ces trois grains dans un petit mortier, avec une once d'eau distillée qu'on y ajoute peu-à-peu, de manière à ce qu'ils soients exactement dissous. On ajoute à cette solution un autre composé d'une once d'eau distillée et d'une once d'alcool purifiée; on agit le tout, et on laisse déposer. On met une seule goutte de cette liqueur bien claire, dans une bouteille contenant trois onces d'eau distillée et une once d'alcool

* Indeed the *only* restriction made mention of is "in some particular cases, where the original disease has been very violent, and advice has been sought for the *after sufferings* too late, ... that belladonna is no longer of service;" but in this restriction we recognise a very great amount of speciousness; what is it but to say that whenever and wherever the disease baffles the belladonna, it is not to be laid to its charge, but to the mistake of a too-late advice or consultation.

† Organon: Nouvelle Traduction, par Jourdan, p. 85.

‡ The title of Schenck's paper, as published in Hufeland's Journal, is "Versuche mit dem Hahnemann'schen Präservatif gegen das Scharlachfieber, von Hrn. Hofrath Schenck."—It is

rectifié: on agite bien le tout. C'est cette liqueur qui sert de préservatif On en donne aux enfans au-dessous de neuf ans une seule goutte, et aux personnes au-dessus, deux gouttes sur du sucre, tous les quatre jours, de manière à ce qu'on reste deux jours pleins sans en donner. . . . . M. Hahnemann me conseilla en même temps de recommander qu'on préservât les enfans de toute commotion vive, ainsi que de lésions externes: mais de ne rien changer d'ailleurs à leur genre de vie. . . . . . Le 7 févrior l'on commença l'usage des gouttes, et on les continua pendant quatre semaines." (p. 391.)

In this experience of Schenck, let it be noticed that three individuals who had taken the belladonna four times were attacked, and let the possibility of the epidemic having approached its termination before his observation of it began, not be lost sight of. To M. Schenck, M. Rhodius writes as follows:

"Altenkirchen, ce 15 Juillet, 1809.

"L'application de la belladone, comme préservatif de la fièvre scarlatine, a eu ici un grand succès. Lorsque je reçus ce moyen, cette dernière régnait déjà fréquemment dans la ville. Les trois enfans de M. l'architecte de Trott etaient dangereusement malades dans la maison de M. le gouverneur de Poelnitz, dont les deux enfans habitaient l'étage au-dessous. On donna aussitôt le préservatif à ceux-ci, et ils ne furent pas atteints. L'enfant de M. Furchel, qui demeurait dans le voisinage, fut préservé par le même moyen. La bonne d'enfant de M. Hertel était très-dangereusement malade; on donna le préservatif aux deux enfans, et ils n'eurent pas la maladie. Une de mes trois domestiques avait également la fièvre scarlatine: les deux autres, quoique habitant la même chambre que la malade, furent garanties de la contagion par le préservatif. Je pourrais ajouter plusieurs autres faits à ceux que je viens de rapporter: mais je regarde cette énumération comme superflue, et crois en dire assez, en affirmant que tous ceux qui ont fait usage du préservatif ont échappé à la contagion.

"Signé, RHODIUS."

MM. Himly* and Hufeland each add a note to M. Schenck's communication: both speak favourably of belladonna as a prophylactic; the former confirms Schenck's observations, but adds no new ones.

The experience of the observers just named, whether contained in Schenck's original paper in Hufeland's Journal,† or as quoted by Bayle, appears to us as scarcely warranting the language which the latter employs in regard to it, and which Dr. Black‡ transcribes. Bayle, let it be observed, gives, in the first place, numerous details of individual experience, and then, as is usual with him, adds a condensed view of the evidence in the form of a report. Now, to say the least, the deductions made by Bayle do, in some instances, scarcely tally with the evidence in detail. We have found it the best way to compare the two, and when possible, to refer to the original paper from which the French physician quotes. The experience of Hufeland and Rhodius is thus given in the report of Bayle,—"gave perfect immunity to all the individuals to whom

from no desire to find fault, but, on the contrary, with great reluctance, that we must, at the outset of our references to Bayle, express our extreme astonishment and disapprobation of the course Dr. Henderson has adopted. He writes at p. 112 of his work—"Before adverting to the experiments made in Edinburgh, *I shall adduce from an article by M. Bayle*," &c &c. Now, it is quite clear that Dr. Henderson has never had recourse to Bayle, but only to Dr. Black's very inaccurate representation of what Bayle has written: for he even copies Black so literally as to transcribe his errors—one of which, miserable as it is, we must beg Dr. Henderson to correct. He follows Dr. Black in referring to the Bibliothèque Thérapeutique, tom. ii. p. 588, *et seq.*, being unaware that there are only 532 pages in the volume.

* Himly, who was professor of medicine at Göttingen, was joint editor, with Hufeland, from 1809 to 1814, of the celebrated German journal which bears the name of the latter.

† Mai, 1812. ‡ Principles and Practice of Homœopathy, p. 36.

they had administered this substance in several very violent epidemics."* We leave our readers to judge whether or not the statement of Rhodius, in the letter already quoted, authorizes the employment of such terms as "gave perfect immunity," and "several very violent epidemics."

M. Masius, Professor of Medicine at Rostock, furnished a paper to Hufeland's Journal in 1813. His belief in the efficacy of belladonna is founded on his own immunity from scarlet fever, when occupied during two years at Schwerin, along with M. Sachse, in treating cases of a malignant type. He took half a grain of the extract every day on which he visited scarlet fever patients, in four doses,—"Et je fus préservé." At another time when, during winter, scarlatina was prevalent at Rostock, both Masius and his children were preserved by attending to the same precautions. We are rather amused at the manner in which M. Masius is prepared to meet any objections which may be offered to his very paltry evidence. "J'aime beaucoup," he says, "un scepticisme raisonnable, mais je déteste l'aveugle incrédulité de notre siècle." We shall have more to say by-and-by of the "hazard" to which M. Masius is aware that some at least may be inclined to ascribe his preservation, and this, evidently, because the narrative favours the author's own purpose.

Gumpert, a physician at Posen, commences a contribution quoted in Hufeland's journal for July, 1818, in very much the same way as some medical men have written during the last few years: he did not, and they have not, given belladonna before, because they wanted "faith," or "confidence," in the discovery of Hahnemann. Gumpert, who was happy in the possession of four children, of the respective ages of thirteen, eleven, seven, and two years, administered belladonna to each during a period of three months, when scarlet fever prevailed as an epidemic in Posen. At one period the disease existed in the same building as his family lived in, on the floor below his own house, and when in every house in the same street there were persons affected with the disease. The elder children attended a public school. The younger and elder children were alike preserved. Gumpert, at the same period, employed belladonna in upwards of twenty families which he attended, and always with success. The preservation of his patients, even in the hands of this most sanguine doctor, was not, however, universal. One person took the disease during the first week of prophylactic treatment, and another, a child, after taking the belladonna for two weeks. We are left to conclude that these were the only two who contracted the disease after taking the belladonna; but we are directly informed that Gumpert never had a case of scarlatina in which the specific had been employed for more than two weeks. We are, moreover, told, that in one family, consisting of six, to which the second exceptional case belonged, one took the disease, and two a few days thereafter became affected with sore throats and slight fever, without having eruption or desquamation.

In his synopsis of Gumpert's report, just as in that of Himly already referred to, Bayle does not adhere to the strict letter of the observer. This is perhaps pardonable in Bayle, because within the four corners of his book the statement of Gumpert is given *in extenso;* but what are we to say of Dr. Black, who has evidently never read the statement of

* Henderson, p. 113.

Gumpert, either in Hufeland's Journal, or *in extenso*, as given in Bayle;* or if he has read either, has contrived to ignore both.†

Gumpert père appears to have been the only one in the same district as his son who employed the belladonna. The latter records his father's success, during some years and in several epidemics, as well as the fact of the confidence of the inhabitants of the district in which he resides being so firm in the belladonna, that the druggists dispensed it without the form of a medical prescription: "et qu'il y a la même confiance qu'en la vaccine."

Gumpert père further mentions, that in no case in which the belladonna has been administered, at the proper time and in the approved manner, has scarlatina declared itself; and that those few cases of the disease which have occurred owing to the belladonna not having been administered during a sufficiently long period, have invariably been of a very mild type. This is no doubt the evidence of Gumpert père; but we profess ourselves entirely at a loss to discover how Bayle, from it, is able to assert that Gumpert, by the timely and judicious use of belladonna, prevented the introduction of scarlatina "*into several villages.*" In this statement Dr. Black of course follows. After this our readers will scarcely require our advice as to the necessity of reference to the original quarter for information regarding the experience of German physicians.

M. Berndt‡ observed an epidemic which occurred at Cüstrin in 1817, 1818, and 1819. The following are the results of his observations:

1. Of 195 children daily exposed to contagion, and to whom I administered the belladonna, there were only 14 who, notwithstanding the remedy, contracted the disease, whilst the other 181 were preserved.

2. The same experiments, made with a solution of three grains of the extract of belladonna, upon a large number of individuals, equally exposed to the influence of contagion, resulted in the preservation of the whole number.

3. The 14 who did suffer had the disease less severely than those who had not been similarly subjected to the influence of belladonna.§

Muhrbeck,|| Dusterberg, Behr, and Meglin are all cited by Bayle as confirming in their own experience the peculiar virtue of belladonna. He quotes on this occasion from Martini's paper in the 'Revue Médicale' for 1824. 'Muhrbeck speaks in the highest terms of its efficacy, having employed it for about seven years, and always with success. In regard to its action he makes the following remark—that vaccination and belladonna differ in the preservation effected by the former being lasting, that of the latter temporary merely. The experience of Dusterberg is important; we shall, therefore, quote it at length from Bayle.

"Pendant trois épidémies consécutives de scarlatine, j'ai employé la belladone

* Taken from Marc's translation in the Biblioth. Méd., tom. lxv. p. 114.

† It is not Gumpert who says he preserved eighty individuals, it is Bayle who supposes most gratuitously that each of the twenty families contained four individuals.

‡ Berndt's paper in Hufeland's Journal for 1820 is entitled, "Bestätigende Erfarungen über die Schutzkraft der Belladonna gegen die Ansteckung des Scharlachfiebers, von Dr. Berndt."

§ We shall shortly have occasion to refer to the strength of the dose of the remedy employed by Berndt and others.

|| The title of Muhrbeck's paper is, "Die Schutzkraft der Belladonna gegen das Scharlachfieber." The same paper, with the author's name changed into Muhskbech, is rendered into French in the Nouveau Journal de Médicine, tom. xii.

avec un succès tel, que je regarde ce remède prophylactique comme aussi efficace que l'inoculation de la vaccine. En effet lorsqu' en 1820 la fièvre scarlatine menaçait la population de la ville Warbourg, je me décidai à vérifier les expériences connues jusqu' alors sur la vertu prophylactique de la belladone. A cet effet je fis prendre aux enfans confiés a mes soins 10, 15, ou 20 gouttes, suivant l'âge, d'une solution faite avec trois grains d'extrait de belladone et trois gros d'eau de cannelle. Cette solution ainsi administrée deux fois par jour, et durant plus d'une semaine, eut pour effet que tous les enfans ayant fait usage du préservatif furent préservé de la contagion, malgré leur contacte intime avec les individus atteints de la fièvre scarlatine. Pour mieux faire ressortir l'effet de la belladone et en écarter celui du hasard, j'ai choisi dans chaque famille un enfant, lequel fut excepté de ce mode de traitement. Or, tous les enfans auxquels l'usage du préservatif était demeuré interdit, furent attaqués de la contagion. Plusieurs enfans, a la vérité, n'ayant usé du préservatif que pendant quatre on cinq jours, furent atteints également de la scarlatine; cependant, presque chez tous, la maladie fut si peu grave, que l'on ne s'aperçut de sa présence que lors de la desquammation." (p. 404.)

The following is the experience of Behr, at Bernbourg, during an epidemic which prevailed in that town in 1820, and which, though at first not of a formidable character, speedily acquired a more fatal aspect. Among forty-seven individuals, including children and adults, to whom the belladonna was given, only six were attacked by the disease, *and in nearly all the six* the disease was of a benign character.* After concluding his account of the experience of Behr, M. Bayle refers to that of Méglin,† at Colmar, who found, during an epidemic which continued during the autumn and winter of 1820, and the following spring, and which at times (*assez souvent*) assumed a severe and fatal character, that all those who, before the invasion of the epidemic, had taken the specific, were preserved. M. Meglin administered the root of the belladonna in powder, with a little sugar, according to the following prescription: ℞. Pulveris radicis belladonnæ, gr. ij.; sacchari albi, ʒij. Misce: et divide in 60 partes equales. From one to five doses to be taken, according to the age of the patient, and to be repeated four times daily.

M. Koehler, physician of Cercle, records the following. A child, one of seven, was attacked with scarlet fever well marked; the other six took a very small dose of belladonna, and were preserved, though remaining in the same apartment as the sick child.

* The paper of Behr is one of the most interesting, if not the most so, of all those published in Hufeland's Journal upon this subject; it contains a table, giving the name, age, date of the commencement of the disease, &c., in 47 cases.—Hufeland's Journal, Stück ii., Aug. 1823.

In Dr. Black's account of this physician's experience, he says, the six alluded to above "were attacked in an almost insensible manner." This is certainly not Behr's own account, as our readers may satisfy themselves, by referring to the paper of Martini in the Revue Médicale for 1824. We confess to feeling a very strong dislike to the frequent discrepancies which we find between the different writers' own accounts—which surely are the accurate ones—and those furnished by Dr. Black; and as Dr. Henderson has rested satisfied by always referring to the latter, and has, in a foot-note to page 115 of his own work, recommended the English reader to the same source, we take this other opportunity of directing him from so unworthy a quarter. It may be, and in most instances is, very true, that the important facts in regard to the question at issue, as given by the German writers, are fairly enough rendered in both Dr. Black's and Dr. Henderson's pages; but we have a right to expect more than that; and from those who ask us to believe experiences in which they put faith, we require that these experiences should be by them truthfully and accurately presented to us, otherwise let them furnish their readers with a simple reference to the authorities, to which, it appears to us, Dr. Black has never once turned for himself.

† See Nouveau Journal de Médicine, &c., Paris, for November, 1821, under the head Variétés, the passage which M. Bayle quotes, and which we have rendered above.

Dr. Beeke, among other experiments in favour of the peculiar virtues possessed by belladonna, mentions that the physician of the district, Wolf, in Silesia, encountered an epidemic of scarlatina in the village of Staedtel; 120 persons were already affected; the specific was administered, and thereafter there occurred 39 mild cases. In two other villages, where 132 individuals made use of the same extract, only 6 were attacked. In 1820, at Siegen, the son of a merchant was attacked with scarlet fever. His aunt, who had paid him a hurried visit, was also seized. She was the mother of three young children; they took the belladonna, and, though they were always beside their mother, they were preserved. Dr. Bénédix employed belladonna with success against the contagion of a malignant fever in the island of Rugen. His paper, a short and interesting one, follows Behr's in 'Hufeland's Journal' for August, 1823: and after it come two notices, one by Dr. Wesener, of Dülmen, in Westphalia, the other by Dr. Zeuch, practising in the Tyrol. The former appears to have thought little of the power of belladonna till he administered it to his own children, and finding them preserved from the contagion of prevailing scarlatina, he changed his views. The latter, in the military hospital for children, had the following experience. Twenty-three children out of 84 became affected; to the remaining 61 belladonna was administered during 20 consecutive days; only 1 of the 61 took scarlatina, although the disease continued to prevail in the neighbourhood of the hospital. Dr. Suttinger reports that before belladonna was administered several persons had died during an epidemic of scarlatina which occurred at Miaskowo, but that after recourse was had to belladonna no other case happened.

Hufeland, the learned editor, commences the November number of his journal for the year 1825 thus:

"Es ist mir grosse Freude, die schützende Kraft der Belladonna gegen das Scharlachfieber durch neue Erfahrungen zu bestätigen. Es sind nun fünf (dreizehn?) Jahre vergangen, dass in diesem Journal die erste Aufforderung zu der Auwendung dieses Schutzmittels erging, und jedes Jahr hat seitdem eine Menge gunstige Erfahrungen geliefert. . . . . . . Ich selbst habe das Mittel mehrmals in meiner Praxis angewendet, und nie gesehen, dass eines von denen, welche dasselbe gehörig gebraucht hatten, angesteckt worden wäre."*

Having passed this panegyric on belladonna, the observations made in the Frederick Institution at Berlin, to which he is physician, by Kunzmann (whom Bayle calls Kunstmann, and of course Black does so also,) are detailed. He had remained doubtful as to the efficacy of the remedy, till, in January, 1825, he became, from his experience in the institution already referred to, satisfied as to the protective virtue of belladonna. In it there were about 70 children of both sexes, from 4 to 14 years of age. On the 25th December, 1824, scarlatina manifested itself in the person of the director's son, and three days later two young girls, one of 4, the other of 7 years, became affected. The

* "It is to me a great pleasure to be able to confirm, by new observations, the prophylactic power of belladonna in scarlet fever. It is now five (thirteen ?) years since, in this journal, the first mention was made of the employment of this preventitive, and each year since that time has brought with it a large number of corroborative facts. . . . . In my own practice, I have on several occasions used this remedy, and I have never seen one of those who used it in the proper manner affected by the disease."

sick children were separated, but, adds Kunzmann, it was impossible to cause a complete isolation. The sound children then received a mixture, composed of two grains of the extract of belladonna in an ounce of distilled cinnamon water, of which each child took as many drops twice daily as he or she had years. From that time to the 23rd of January, a period of four weeks, no case presented itself, but on that day a little boy of ten became affected, but only very slightly, proving that the contagion still existed in the house. A second son, however, of the director of the institution, who had not taken the mixture, suffered a severe attack of the disease. During six weeks the remedy was persevered in, and no cases occurred. The table furnished by Gelnecki, of Stettin (with whose name also Bayle, and Black after him, take great liberties, manufacturing it into Geneki), succeeds Kunzmann's report, and is a remarkably interesting one. His experience was obtained in Glasow. There were in all 94 children. Of these 76 appeared to be preserved from the contagion by the use of the belladonna, while 15, who had not employed the remedy, became affected with the disease, 3 who had employed the belladonna took scarlet fever, and 2 of the 3 died. Of the 15 who took the disease without having made use of the prophylactic, 4 died.*

Maizier,† district physician of Burg, made use of belladonna in the village of Nigripp, and not one of the 170 children to whom he administered it became affected with scarlatina. The treatment was continued for 14 days, and then the epidemic disappeared, though in the neighbouring village of Detershagen, where no belladonna had been employed, it continued to prevail, and some children died. This physician had previously obtained similar results with belladonna in 1821: an epidemic of a fatal character prevailed at the village of Grabow, and its cessation followed the use of the prophylactic. In the districts of Riesel and Ziegelsdorf, where some children had been already seized, the belladonna was employed, and no other case occurred. Also in Burg, the place of his own residence, among from 60 to 70 children, there were only 3 or 4 who became affected with scarlet fever (when epidemic) after the use of the specific. Hufeland mentions Dr. Wiedemann, of Wolmirstedt, as bearing like testimony.

Dr. Randhan,‡ in the Orphan Hospital at Langendorf, on the occurrence of 2 cases of scarlatina, gave the belladonna to the 160 remaining, from February (when the two cases presented themselves), so long as the contagion lasted. On the 21st of April, the disease had attacked none of the other orphans, not even two who shared the same apartment with the two previously sick children. Velsen,§ physician at Clèves, reports, that of 247 persons who used the belladonna, 13 only contracted the disease, of whom 4 were children who had taken the remedy during several weeks,

* The inexcusable blunder which both Bayle and Dr. Black, in copying him, have committed, in quoting the testimony of Gelnecki, is also observable in the table which Bayle has prepared, but which, from the inaccuracy we allude to, is rendered useless. Dr. Black, in a foot-note to page 39 of his book, says, "there is an error here as to the number, also in the tabular list;" but he had not the ingenuity to correct the mistake into which Bayle had fallen, although in Bayle's own work the opportunity for so doing was afforded him.

† Hufeland's Journal, Nov. 1825. For his account of Maizier's experience, Bayle (and it is singularly inaccurate) quotes from a French journal. Journ. des Prog., tome i. p. 242.

‡ Hufeland's Journal. 1825.

§ Journ. Complémentaire du Dict. des Sciences Méd., tome xxviii. p. 370.

but not with regularity, 1 child who had taken it regularly during fourteen days, another during eight days, and the rest during forty-eight hours. In all the cases the disease was mild, milder than with those who had not taken the medicine. Among the facts mentioned by Velsen is the following: A man, the father of four children, who had visited but only for a few seconds a friend labouring under scarlet fever, was seized, some days thereafter, with the same disease, and in a violent manner; his wife and children, the youngest of whom was only three weeks, and the oldest four years, took with great regularity the extract of belladonna, and, although day and night were passed with the sick husband and father, and in a small and badly-ventilated chamber, none took the disease. M. Velsen adds: "Est ce là l'effet du hasard, ou le résultat de l'emploi de la belladone?"*

Such are some—indeed, nearly all—of the testimonies borne by foreign—and more particularly by German physicians—to the prophylactic virtue of belladonna. We now proceed to consider the facts which have been advanced in this country; here we find the evidence neither so extensive nor on so large a scale. The following account is given by Messrs. Taynton and Williams, gentlemen practising at Bromley, in Kent, in 1829:†

"During the months of April and May, the scarlet fever was very prevalent in this town and neighbourhood, and in many cases it proved fatal. Our attention was called by a friend to a notice in the 'Lancet'‡ of the 2nd of May, 'On the prophylactic powers of belladonna against scarlet fever, by M. Hufeland.' We were at that time attending in a boarding-school where the disease had attacked 12 of the boys, many of whom had been most dangerously ill, but none had died. There still remained several boys (perhaps 20) who had not taken the infection; also 4 young children of the master's, and several servants. We immediately commenced the use of the belladonna, in the exact manner and dose advised by Hufeland. Only 6 or 7 persons in the house took the disease afterwards, and in every instance it assumed the mildest form.

"In another school, we were called to visit a child about two years old, who had been attacked the evening before. The disease was of the most malignant character, and the child died on the following morning, the third day from the attack. The house is a very small one. There were in it 3 other young gentlemen and 5 boarders, and a servant-girl. The belladonna was faithfully administered, and not one individual took the disease. We will not offer any conjecture on the *modus operandi* of the belladonna, or whether it did or did not prevent the other members of these families from taking the disease. The facts are stated exactly as they

* Did our space permit, we might have quoted Wagner's report of the epidemic at Schlieben, of Dr. Peter's at Leopoldshagen, of Dr. Reuscher at Stendal, and Dr. Cohen. For these we beg to refer the reader to Hufeland's Journal, 1825, also to the Gazette de Santé for the same year, for the statement of M. Lemercier. These are all alike favourable to the theory of the prophylactic power of belladonna.

† The London Medical Gazette, vol. iv. p. 297.

‡ The following are the conclusions of M. Hufeland, contained in the paper which Messrs. Taynton and Williams refer to:

I. The proper use of belladonna has, in most cases, prevented infection, even in those instances where, by the continual intercourse with patients labouring under scarlet fever, the predisposition towards it was greatly increased.

II. Numerous observations have shown that, by the general use of belladonna, epidemics of scarlet fever have actually been arrested.

III. In those few instances where the use of belladonna was insufficient to prevent infection, the disease has been invariably slight.

IV. There are exceptions to the above three points, but their number is extremely small.—Lancet, May 2, 1829.

occurred, and we entreat our professional brethren to make trial of the belladonna whenever a favourable opportunity occurs."

The following is the result of Dr. Black's* experience :

"Belladonna was administered to 11 children who never had scarlet fever, and who were living in a house with 2 cases of scarlet fever, the one of them attended with sloughing sore throat, and in intercourse with these cases: all escaped, even one who was sleeping in the same bed with one of the patients. In another instance, we gave belladonna to 4 children, none of whom had the fever, and were directly exposed to the contagion; 3 escaped; 1 took the fever, but so slightly, that we were inclined to regard the symptoms as those of belladonna. In another instance, we administered the remedy to 4 children and an adult, who were living in the same house with 2 cases of scarlet fever. The adult and 2 children were seized with the fever; 2 had only taken the remedy for two days, and 1 for three days; the other 2 children escaped. The 3 cases were much milder than the 2 cases in which no belladonna had been given as a preservative. Out of the 20 cases, we observed the remedy produce headache, with increase of pulse, in 1 child; in another, there was slight redness of the skin, which lasted for eight hours, and unattended with fever."

Dr. Patrick Newbigging† writes as follows:

"Scarlet fever having prevailed in John Watson's Institution to so considerable an extent, and the cases having occurred in close succession, notwithstanding a system of separation as complete as was possible amongst inmates residing under the same roof, I felt desirous to try the effect of belladonna as a prophylactic against the disease. It was an opportunity such as rarely occurs for the investigation of the alleged virtue of this drug on a large scale. Having ascertained the number of children unaffected with scarlet fever, or who were uncertain as to ever having had it—making, in all, 69—I directed that belladonna should be administered to them, in the proportion of one-sixth to one-fourth of a grain twice a-day, according to the age of each child; the first dose being given before breakfast, and the last dose at bedtime. This plan was adopted on the 16th of October. Three new cases occurred between that and the 20th. After that date no child was affected, nor has there been any instance of scarlet fever since that period in the institution. . . . . . I should now consider it my duty to lose no time in making use of this medicine on the first appearance of this disease, and I would strongly recommend the same plan of practice to those of the profession who are connected with similar educational institutions, with the view, not merely of attempting to ward off a malady so uncertain in its progress, and occasionally so fatal in its termination, but also with the object of accumulating information on a point of such paramount importance to the public health. The opinion I have adopted on this point has been greatly strengthened by a similarly beneficial result produced some time afterwards in another case. I was requested to visit a young gentleman at a large educational seminary. I found him labouring under scarlet fever, with profuse eruption, an aphthous and very painful condition of the throat, accompanied by all the usual symptoms exhibited in the acute stage of a smart attack of this disease. I caused my patient to be removed, a few hours after first seeing him, to the house of a relative, and placed his brother, who continued to reside in the seminary, upon belladonna. This treatment was adopted on the other members of the family, consisting of 19, who had not previously been affected with scarlet fever. No other case occurred."

We might easily multiply the quotation of experiences such as the three now adduced. We believe such a procedure, however, to be unnecessary, the facts in favour of the employment of belladonna being as

* British Journal of Homœopathy, vol. i.
† Monthly Journal of Medical Science, Sept. 1849.

strongly elicited in these three as in any other recent accounts we have met with.

Our readers, after having followed us in the production of these various facts and opinions of authors in favour of the prophylactic action of belladonna, will naturally expect us to advance the facts and opinions of a contrary bearing. And if we now limit ourselves to the quotation of a few of the former, and to a mere glance at the general nature of the latter, it must not be supposed either that the facts are wanting or are even limited in number, or that silence has prevailed over the expression of opposite views. Such is certainly not the case; there exist, if not so many facts as in favour of the prophylactic action of belladonna, at least stronger, and altogether more reliable ones, on the inefficiency of its employment; while the expression of opinion in regard to its inefficiency —not always formed on the justest grounds, we allow—have undoubtedly been neither few nor uncertainly declared. Among German writers who have adopted this view is Lehmann, the staff physician of the garrison at Torgau. Dr. Black makes it appear as if Bayle objected to the evidence of Lehmann, on the score of its being "supported by no facts." Such is not the case; Bayle never could have made such a mistake with the paper of Lehmann before him, and when he writes,—"Nous ne pouvons apprécier à leur juste valeur l'opinion de ces auteurs, parce qu'elle n'est appuyée d'aucun fait, et que la maladie n'est point décrite," Bayle means this to apply to the opinions advanced by Raminski* and Teuffel,† as quoted by Barth. Any reader, however, of either Black or Henderson, will come to the conclusion, that by Bayle the evidence of Lehmann was held in the same estimation as that of the two other observers just named,—affording another proof of the danger of trusting to second-hand reading, and of the propriety of consulting in all cases, where possible, the original statements of every author. Had Dr. Black not rested satisfied by quoting the mere *resumé* of Bayle, he would not have fallen into this error; for at page 417 of his same volume, Bayle devotes a paragraph of nearly half a page in length to Lehmann's observations, entitling them, 'Observations du Docteur Lehmann:‡ Epidémie de Scarlatine dans laquelle la Belladone ne prevint pas la Maladie.' The title of the paper itself, in 'Rust's Magazin,' is different; it is given below.§ What Bayle says of Lehmann's experience is in every respect fair, and when he expresses his opinion in the following words, "Il n'a jamais pu parvenir à empêcher la contagion chez eux qui y étaient disposés, ni a modérer la gravité de la maladie chez eux qui déjà en étaient atteints," (p. 417,) he says *no more than* Lehmann's accurately observed and precisely stated facts required.

Now this paper of Lehmann's is both a very interesting and a very important one. His experience was large; his attention to the mode of preparation and the manner of administering the belladonna were alike

* Raminski is mentioned by Barth to have lost his own son, and to have afforded many proofs of the augmentation of the disease after the employment of belladonna.

† Teuffel's observations, says Bayle, are to the like effect.

‡ Magazin für die gesammte Heilkunde, Herausgegeben Von Dr. Johann. Nep. Rust., vol. xxi. 1826 (at p. 42).

§ Die Unwirksamkeit der Belladonna als Schutzmittel gegen das Scharlachfieber, nebst einem Impfungsversuche dieser Krankheit. Von Dr. Lehmann.

exact—("en le donnant," Bayle himself says, "suivant toutes les règles indiquées par ceux qui ont préconisé ce moyen,")—and lastly, his memoir has the advantage of almost all others which we have perused, while it is inferior to none in exhibiting the precision of its author's observations. We shall quote four of these:—

1. In a family consisting of three boys, the eldest was attacked with scarlet fever. The two others were immediately removed from the sick boy, and were confined to the floor of the house below that on which his room was. They got, at the same time, every morning and evening, the belladonna solution. After this boy's recovery, and at the end of one month from the first appearance of the disease, he was restored to the society of his two brothers. Four months later the youngest brother was seized with the disease in a severe form; he recovered, and then the third (in respect of years, the second) brother, who remained on this occasion in proximity to the patient, but at the same time took the belladonna regularly, contracted the disease on the tenth day, and fell a victim to it.

2. In a family consisting of five brothers and sisters, a boy of five years was first attacked with scarlet fever. To the other four the belladonna was immediately given. After eight days a little girl of four years old was seized, and on the third day of the disease died. The following day a sister of three years of age took the fever mildly, and recovered; another sister, of eleven years, was almost immediately afterwards affected, and on the fourth day of her illness died. The eldest brother, long a sufferer from bad health, and particularly from a chronic affection of the heart, remained free from the disease. It is of importance to know that the four patients together occupied a small and extremely damp room, on the ground floor; and this, indeed, was accepted as the probable cause of the early deaths.

3. A boy of five years, an only son, contracted scarlet fever after having uninterruptedly, during several months, taken belladonna. The fever assumed a cerebral character, and on the fourth day the little patient died.

4. In a family of four children, the eldest (who was five years) became affected with scarlet fever. The remaining three were immediately put on the belladonna; two of these, on the twenty-first day of the employment of the drug, became affected with the disease in a severer form than the first child, who had taken no belladonna.

Along with other facts of a like nature, Lehmann mentions that, in his own experience, whole families (one in particular, consisting of seven children,) altogether escaped the disease, though epidemic in the place where they resided.

At Stralsund, writes Barth (quoted by Bayle, p. 419), Dr. Mierendorf observed that the children to whom belladonna was administered became more seriously affected, and died in much greater proportion than those for whom the drug was not prescribed. Dr. Schmidt, writes the same authority, lost two children who had taken the so-called prophylactic. Of 100 children so treated, fifteen became affected with scarlet fever, and one died.

Dr. Raminski, who lost his own son, had so many proofs of the exacer-

bation of the disease during the employment of the belladonna, as to make him altogether doubt its efficacy as a remedy.

Mr. Benjamin Bell, in the course of an article on "Scarlet Fever as it appeared in George Watson's Hospital in the Spring of 1851,"* writes as follows:

"Conceiving that no means for arresting the disease ought to be neglected, and that a favourable opportunity now offered itself for testing the alleged prophylactic virtue of belladonna, I determined to give it a full and fair trial.

"Accordingly, on the 21st of February, upon the appearance of a second case of scarlet fever, the fifth part of a grain of the extract was given, morning and evening, to each of the boys. The dose was found, in a few days, to be too large, from the dilated state of the pupil and impaired vision which it occasioned in several instances. It was accordingly diminished, and then administered without interruption, to all the boys, who continued well until the 7th day of June, a full month after the last case of scarlet fever had occurred. It is important to remark, that the second case already referred to had been in the sick-room, separated from the rest of the boys, for more than a week before the symptoms of scarlet fever appeared, and that no additional case occurred until the 21st of March, an entire month after the belladonna had been regularly administered. There was thus ample time for the manifestation of its virtue as a prophylactic; but the subsequent occurrence of so many cases seems to throw considerable dubiety over the existence of any such power. No experience of a merely negative character can be regarded as of much weight, when contrasted with this positive experience now detailed. It is by no means unusual to meet with only two or three cases of scarlet fever in a large assemblage of children, without the belladonna having been used at all; and therefore we are not called upon to give it the credit of securing a similar exemption in cases where it has been administered; but surely the occurrence of 23 cases out of 54 boys, who might be legitimately reckoned liable to the disease, is an overwhelming evidence on the opposite side."

In reference to the prophylactic action of belladonna, we find Dr. Elb, a homœopathic practitioner at Dresden, writing as follows:†

"I must add, that in general I did not find the prophylactic power of belladonna by any means so generally borne out; although cases have come before me, in which I gave belladonna as a preventative, and the children to whom I administered it remained free from scarlet fever. But just as often have I found that children have been attacked by it, notwithstanding the use of belladonna for several weeks, and that this long previous use of the belladonna had not even the power of diminishing the violence of the disease."

The interesting experiments of Dr Balfour, conducted at the Royal Military Asylum at Chelsea, are thus alluded to by Dr. West:‡

"I cannot do better than relate the experiment in the words in which Dr. Balfour was good enough to communicate it to me. Scarlet fever having broken out in the visitation, Dr. Balfour determined to try the virtues of belladonna. 'There were,' he says, '151 boys, of whom I had tolerably satisfactory evidence that they had not had scarlatina. I divided them into two sections, taking them alternately from the list, to prevent the imputation of selection. To the first section (76) I gave belladonna; to the second (75) I gave none; the result was, that *two* in each section were attacked by the disease. The numbers are too small to justify deductions as to the prophylactic power of belladonna; but the observation is good, because it shows how apt we are to be misled by imperfect observation. Had I given the remedy to all the boys, I should probably have attributed to it

* Monthly Journal, Aug. 1851.

† See British Journal of Homœopathy. 1849. Vol. vii. p. 38.

‡ Lectures on the Diseases of Infancy and Childhood. Third edition, 1854.

the cessation of the epidemic.' To these remarks," continues Dr. West, "I need add nothing. They convey a most important lesson, but one which, I fear, we are all too apt to forget in the study and in the practice of medicine." (p. 600.)

Dr. Andrew Wood's experience in Heriot's Hospital is thus mentioned by Dr. Simpson:

"In Heriot's Hospital my friend, Dr. Andrew Wood, placed half of the boys in each ward or sleeping division on belladonna, and left the other half without any such protection. The disease did not spread much; but at least as many of those using the belladonna as of those not using it were attacked; and the only fatal case out of 40 which occurred during that epidemic, was that of a boy who had been using belladonna in doses of one-eighth of a grain twice a-day for three weeks previously to his being attacked."*

In drawing this article to a close, we have to consider, first, whether or not the prophylactic action of belladonna against scarlatina, as claimed, promulgated, and practised by Hahnemann and his followers, has stood the test of experience, and is now to be regarded as a reality; and second, whether, in regard to the employment of belladonna for a like purpose, in larger doses than those recommended by the former, experience and observation lead us to embrace the practice as a real boon to humanity, or to abandon it as inefficient and absurd.

At the outset, let us exactly understand what Hahnemann did, and his followers do now, claim for belladonna as a prophylactic: and what was the manner of its exhibition which he advised and required. This inquiry is not unnecessary, when we consider that in the numerous instances of failure of the belladonna reported to Hahnemann himself, he invariably attributed the want of success to the prophylactic having been employed in cases of a fever different from scarlet fever;† or the *Miliaire pourprée*, which was, according to Barth, imported from Holland, in the

* Through the kindness of Dr. Wood and of Dr. Simpson, we are enabled to give the experience of the former a little more in detail. The plan Dr. Wood adopted was an excellent one, and the result of his experiments, taken in connexion with Dr. Balfour's and Mr. Bell's (somewhat differently performed), to our mind appears conclusive: "The plan that I proposed to myself," says Dr. Wood, "was this—viz.: whenever scarlatina appeared in any particular ward, and not till then, I immediately made inquiry, and having ascertained the boys who had previously had the fever, these I left out of the question. I then divided the remainder into two nearly equal sections: to one I gave one-eighth of a grain of belladonna, twice a day; to the other, no belladonna was given. This experiment was continued for several weeks, and the reason why it was then discontinued was simply this—that a fatal case occurred in the person of a boy (J. B.) who had been taking the belladonna for nearly four weeks. Taking alarm, I resolved to discontinue the experiment." The following is a brief analysis of the trial:

First ward—containing 11 boys. Case occurred April 17th: 3 already had scarlatina; 5 boys got belladonna; 2 got no belladonna. One of the 5 took scarlatina June 2, and died on the 7th. No other case.

Eighth ward—containing 20 boys. Case occurred April 25th: 7 already had scarlatina; 5 got belladonna; 3 got no belladonna. No subsequent case.

Fourth ward—containing 25 boys. Case occurred May 9th: 4 already had scarlatina; belladonna given to 10; no belladonna to 10. On 19th May, J. G., who had accidentally slept in the same room as a boy who had scarlet fever, and had been taking the belladonna since the 28th April, became affected with the disease in a moderately severe form: he recovered. On 4th June, a boy, who had taken no belladonna, contracted the disease in a very mild form. No subsequent case.

Fifth ward—containing 18 boys. Case occurred May 23rd: had had the disease, 4; took belladonna, 6; took no belladonna, 7. No subsequent case of fever.

Seventh ward—containing 36 boys. Case occurred May 28th: had had scarlet fever, 6; took belladonna, 18; took no belladonna, 11. No subsequent case.

† Hahnemann also speaks of the introduction of this Fièvre Miliaire Pourprée—(or, in German, Rothe Friesel, Purpurfriesel, Roodvonk)—purple rash—as having been introduced from Belgium in 1801. See Reine Arzneimittellehre. Von S. Hahnemann. Vol. i. p. 15.

month of January, 1801. It appears to us that in all probability the limitation of the use of the drug to the cases of scarlet fever, as described by Sydenham* and Plenciz,† was an after thought, and that, notwithstanding the allusion made by Hahnemann to the similarity borne by the epidemic of scarlatina at Königslutter to the disease described by Plenciz; for, unquestionably, in the whole length of the article 'On Scarlatina,' as translated by Dr. Dudgeon, there does not occur a single expression whereby we are to understand that his proposal of belladonna as a prophylactic, or as a remedy strictly so called, was to be confined to cases of the same nature as those which occurred in that epidemic.‡ Were any further proof of this (than the absence of any restriction) required, we think it supplied in the fact of Hahnemann, in his first publication, expressing his belief "that a similar employment of belladonna would also preserve from measles."§ Now, no one will pretend to urge that a closer resemblance subsists between measles and scarlatina, than between that disease and the *miliaire pourprée*, which, if it were really not a variety of scarlatina, must have very closely approached it in character, before so many observant physicians could have been deceived. In our opinion, then, the plea of want of success on the ground of dissimilar diseases being treated under the belief that they were alike examples of scarlatina epidemics, does not hold good; for, first, there is no proof of the disease treated by Raminski, Teuffel, and other physicians, not having been a true scarlatina; and second, Hahnemann himself did not confine the virtues of belladonna to scarlatina, but extended them to a disease whose characters are by a still longer way removed from it than the *miliaire pourprée*—namely, measles.

It has been contended, and this argument is referred to by Professor Henderson, that in instances of failure, another drug than belladonna, dulcamara for example, may have been used. Now, we beg to submit that if this idea is allowed to have any weight, it must be permitted, in

* Processus Integri. (Sydenham Edition of Works.) Vol ii. p. 242.

† Tractatus de Scarlatina. Autore Marco Antonio Plenciz. Sectio ii. Vienna, 1772.

‡ Dr. Henderson argues that Hahnemann has the advantage of Jenner, in not claiming universality of exemption from scarlatina after the use of belladonna, as he says Jenner did after vaccination, from small pox. To us it appears, that if Hahnemann had adhered to his original opinion—from which he at the time allowed no exception—he would have been both more honest and more entitled to our attention. We can, however, see no parallelism, either between the discoveries of Jenner and Hahnemann, or between their subsequent histories.

§ Hahnemann, in adverting to the subject of the treatment of scarlet fever as recommended in the works of various authors, makes this singular admission. "Here we often see the *ne plus ultra* of the grossest empiricism: *for each single symptom a particular remedy* in the motley, mixed, and repeated prescriptions; a sight that cannot fail to inspire the unprejudiced observer with feelings at once of pity and indignation." We think "the cap fits" most exactly here, and even pinches, though Hahnemann, with strange perversion of observation, does not appear to feel it. The proposal and employment of belladonna in scarlet fever is as apt an illustration as could be imagined of the fitting remedy to symptom. Belladonna produces a scarlet rash; therefore, concludes Hahnemann, it will cure scarlatina, or is homœopathic to it. But *scarlet rash* is not scarlet fever, it is only a symptom of it; and if we were to give belladonna as often and as long, and in whatever doses we chose, we might kill our patients, but we never could contrive to give them scarlet fever. The question of the power of belladonna to produce the rash which is so universally believed to follow its continual administration, itself requires revision. For our own part, we can say that, after giving belladonna for a long time, in more cases than one, we have failed, though careful and repeated in our examination, ever to discern it. We do not mean to doubt the production of what may be called *spots* in some cases, but we altogether disbelieve the fact of even these following in any large number of instances. Schultz has justly remarked, that similarity of symptoms, not of diseases, lies at the base of all the therapeutic proceedings of Hahnemann and his followers—a pity they do not see it.

all justice, to affect both sides of the question; for we know no reason, and no experience, to justify such reason,—why Hahnemann and his followers should have always hit upon the proper plant, and those who opposed his views have seldom or never done so. If Dr. Henderson insists upon this point, we are quite ready to allow that some physicians, who employed dulcamara, or some other member of the Solanaceæ, instead of belladonna, have failed (though not in consequence) to protect their patients from scarlet fever; but then, we must contend that certain other physicians, who administered these drugs instead of belladonna, have succeeded in the desire to have their patients preserved from the disease though not in consequence. Let it, however, be remembered that belladonna possesses a singular property—a property almost peculiar to itself—and then we think it will be granted, that any physician entering on a careful investigation into its properties, by means of a given portion of its extract, will first establish the power of that individual specimen to dilate the pupil in the peculiarly marked manner which belladonna does.

Some singular discrepancies exist in regard to the frequency of the administration of belladonna, as recommended by Hahnemann: for example, Barth, as cited by Bayle, says that "tous les six à sept jours" was the interval at which he advised the dose to be administered. Jahr* also says, as expressing the views of Hahnemann, "To this effect the smallest dose of belladonna ought to be† given every six or seven days." On the other hand, in his communication to Dr. Schenck, already quoted, Hahnemann says distinctly, on every fourth day the belladonna solution should be taken; and in his own pamphlet, as translated by Dr. Dudgeon,‡ he condescends to a greater particularity, and orders a dose to be given every seventy-two hours. It is not for us to reconcile these discrepancies, believing, as we do, that it certainly makes very little difference whether the $\frac{1}{432000}$th part of a grain of belladonna§ be taken every seventy-two hours or every seven days. Some of his followers, moreover, take what we should have thought to be unpardonable liberties with Hahnemann's directions; of these we need only cite Dr. Black, who has the effrontery to double the Hahnemannic dose (making it thus $\frac{1}{216000}$th part of a grain), and to make the interval of its exhibition from ten to fourteen days.‖ Surely when Hahnemann's own followers, acting on their own responsibility, double the strength of his remedial measures, and fix their own time for their administration, Dr. Henderson¶ need not be so very indignant at Mr. Bell likewise choosing the amount of his dose, and for himself determining when and how often to employ it.

But leaving the adherents of Hahnemann's system to reconcile these differences, we come to a point in the argument concerning both him and them, which we shall take the liberty of settling for ourselves. Hahnemann distinctly says (as we have already quoted) that the peculiar action of belladonna *does not last above three days*, and the repetition of the dose

* Jahr's New Manual of Homœopathic Practice. Edited by A. G. Hull, M.D. Article, Belladonna, in Symptomatology, p. 161.

† Die von mir gefundene Schutzkraft der Belladonne in der Kleinsten Gabe aller, 6, 7, Tage gereicht, &c. &c., are Hahnemann's own words.

‡ Op. cit., p. 438.

§ And this (for a preventative object) as a dose for a child of ten years, is, according to Hahnemann's own experience, too large.

‖ Dr. Black in the Homœopathic Journal, vol. I. p. 188.

¶ I am persuaded, says Dr. Henderson, Mr. Bell will pardon me for asking if he made him-

of the prophylactic at the end of every seventy-two hours is, therefore, strongly insisted upon, and (though he has mentioned longer intervals) appears always to have been acted upon. He never allows seventy-two hours to pass without the administration of a dose, though, if the epidemic of the disease be very violent, he counsels the safety, if children could bear it, of giving the second dose twenty-four hours after the first, the third dose thirty-six hours after the second, and the fourth forty-eight hours after the third; thereafter to let the subsequent doses be taken every seventy-two hours until the end, in order that the system may not at first be taken by surprise by the miasm.* Now, if we inquire the reason of the period of interval of the doses being at all events limited to seventy-two hours—never allowed to exceed that period—we are met by the (homœopathically speaking) very sensible answer, that "the peculiar action of this plant does not last above three days." Now, if that was Hahnemann's opinion—and these are his *ipsissima verba*—we should like to know what believer in the homœopathic action of medicines has any right to dispute it? besides, Hahnemann first proposed belladonna as a prophylactic in scarlatina, and—to use the argument adopted by Professor Henderson, in addressing himself to Mr. Bell's experience—therefore try his way of it, and adopt his theory regarding it, else leave it altogether alone. But we do not require to do this; all the followers of Hahnemann acknowledge that the period of the duration of the effects of the "divine" remedy never exceed seventy-two hours. "Vis per 56 horas ad minimum, per 72 horas ut plurimum durat," says the founder of the homœopathic school; and it makes very little matter if Hahnemann's ignoble editor, Dr. Quin, daring to attempt to improve upon what he has said and done, adds, in a note, "Aliquando belladonna ad diem vigesimum primum et ultra (most convenient) vires retinet;†" or that Jahr still further improves upon both by saying that the duration of the action of belladonna extends "from one day to eighteen months."‡ If, then, Hahnemann's idea be correct, that the action of belladonna as a prophylactic against scarlatina is exerted only, at the farthest, for a period short of seventy-two hours, we are fairly entitled to conclude, that all these instances of preservation from the disease in which the drug was exhibited at intervals exceeding that space of time, and which have been attributed to its prophylactic action, are just as likely due to any one of the many other causes which may be presumed to have acted beneficially in contributing to the exemption, and to some of which we shall presently allude. We shall, assuredly, not insult our readers, nor these pages, by inquiring if the exhibition, according to Hahnemann's direction, of the $\frac{1}{432000}$th of a grain of belladonna, at intervals not exceeding seventy-two hours, can, or ever has, preserved those exposed to the contagion of scarlet fever; we can unhesitatingly answer the question we put to ourselves in

self acquainted, before he began his researches, with Hahnemann's instructions as to the proper dose, and the interval that should elapse between the successive repetitions of it? If he did not, why try the medicine at all; since there was no other discoverer of the alleged preventative power of belladonna than that same Hahnemann, who also says, that the dose ought to be very small, and ought not to be repeated above once in two or three days? We should prefer Dr. Black's answering this question.

* See Dr. Dudgeon's Translation, p. 439.

† Fragmenta de Viribus Medicamentorum positivis sive in sano corpore humano observatis, a Samuele Hahnemann, M.D. Edidit F. F. Quin, M.D., p. 21.

‡ Hull's Jahr, p. 161.

the negative. And now we pass on to the second. It may, perhaps, have occurred to our readers, that if we proposed to ourselves the settlement of this question by reference to the numbers of the facts and illustrations we advanced, that we were not doing ourselves justice, but that, on the contrary, we were assigning to the believers in the prophylaxis of belladonna an easy triumph. We beg to remind our readers of our expressed determination to give our opponents fair play; and further, of our own acknowledgment that we have been content to adduce a few of the many instances of failure of the drug in the hands of experienced and competent observers. Now, we are ready to acknowledge that, in attempting to determine for ourselves the right of the question we have proposed, we escape from none of those difficulties which all are ready to acknowledge perplex the path of him who, by a reference to the experience of several or of many, endeavours to determine a question in therapeutics. We do most entirely agree with Dr. Alison, who, in the course of an article in this journal,* replete with valuable information and the soundest reasoning, writes:

"We do not mean to deny that questions occur in therapeutics, likewise, as to which large numbers of cases may be compared with advantage, and the 'numerical method' applied, ... but we think it is reasonable and right for practitioners to build their opinions, as to the powers of a remedy, on observations of very different kinds, besides the mere enumeration and statement of ultimate results of the cases in which it is given; or, as it is shortly and justly expressed by a practical author, that, in order to make up our minds as to any such question, it is better, in general, to watch than to count."

Now, the great objection we have to the evidence which has all along been adduced in favour of the employment of belladonna in scarlatina is precisely, that "counting" has taken the place of "watching." Numerous circumstances in the particular epidemics, and in the particular cases occurring in these epidemics, have either been altogether passed over, or, at least, have not been awarded the importance they deserve. If 100 children have been exposed to the infection of scarlatina (the degree of exposure is seldom noticed, or, at all events, is very inadequately described), and belladonna has been administered; and of the 100, if either all or a very large proportion have remained free from the disease, the *post hoc ergo propter hoc* has been, in every instance, adopted; and, apparently, as if there could be no objection offered to its adoption; because belladonna was taken and the 100 children preserved—*therefore* the belladonna preserved them. Again, and irrespective of the necessity of attention being paid, in such investigations, to the minutest particulars, to which we shall presently refer, let it be held in remembrance, that the great majority of facts which have been advanced in favour of belladonna are entirely of a negative character, while those we have brought forward in opposition to it are positive.

"I conceive," writes the lamented Dr. Pereira on this point, "twenty cases of failure are more conclusive against the opinion than one thousand of non-occurrence are in favour of it. .... The cases which I am acquainted with are decidedly against the efficacy of the remedy."†

Let us very shortly glance at some of the foreign cases, and then more particularly consider what Dr. Henderson calls the "Edinburgh experi-

* January, 1854.

† Elements of Materia Medica, 2nd edit., vol. ii. p. 1233.

ments." Now as regards the former, there is not one favourable to the belladonna theory which in the least degree approaches to the "experimentum crucis," and not one of *any weight* at all; if, perhaps, we except the experiment of Dr. Dusterberg at Warbourg, the principal features of which have been already detailed. The choice of an individual in each family to whom the drug was not administered, and the subsequent infection of the *whole* so excepted, is, we acknowledge, a startling circumstance. *Still* we are not satisfied, even supposing the statements of Dr. Dusterberg to be perfectly accurate, that the mode of experiment was a good one. The exemption from the belladonna treatment of one half of each family would have been much fairer; the exemption of only one in each family would undoubtedly serve as predisposing the ones so exempted to contagion in another and very marked manner, which is not even alluded to by the experimenter: the mental influence exerted over the exempted child of each family, we hesitate not to say, would be very decided, and all in favour of his or her contracting the disease. But apart from this consideration, there are points in the narrative of Dr. Dusterberg which make us very sceptical as to the reality of his experiments. Unlike most authors, he speaks of a "*contact intime*" between those who had taken the belladonna and those who were affected by the disease. And further, he asserts, that in most instances of those subjected to the belladonna treatment, at the end of some days there appeared "*a general eruption resembling that of measles*," and that all who presented this appearance remained free from the disease. We take leave to doubt the reality of the above altogether; indeed, as we have previously stated, it is *exceedingly doubtful* if any eruption over the skin of any kind whatever follows the internal employment of belladonna. Some of the other experiments, whose results appear in favour of the prophylactic action of belladonna, seem, as far as numbers are concerned, to tell well. Of 195, 14 attacked, 181 preserved (Berndt's experience). Of 525, 522 preserved, only 3 attacked (Schenck's experience). Out of 20 families, 2 attacked (Gumpert's experience). But apart altogether from any favourable circumstances, which, it is not unreasonable to suppose, may have existed in these cases, and of which the exemption of so large a number as 181 out of 195, and of 522 out of 525, renders all the more likely, are not facts of this kind allowed every day to pass under our eyes, and to attract our attention, while they only, and very properly too, elicit the remark, that these are unusual, or, at the most, remarkable coincidences? The truth is, in regard to scarlet fever, as well as many other infectious diseases, that an amount of capriciousness so evidently attends their progress, indeed, if we might so speak, regulates their progress, as to make it a very difficult matter to decide if, at any time, or in any degree, their occurrence is at all affected or moderated by external circumstances; and if this be true, as undoubtedly it is, how far more difficult must it be to decide if the exhibition of any prophylactic means does good?

Vaccination in its effects made itself at once recognised, and the contrast between the ravages of small-pox at the commencement of this century, and the almost entire immunity from that disease in an epidemic form, which now prevails, are facts so plainly recognisable, and so appreciable, as in the instance of that disease entirely to remove the difficulty referred to. It is altogether otherwise with scarlatina; notwithstanding

the introduction of belladonna, and its extensive employment, both in this country and abroad, as a prophylactic against scarlet fever, we are not aware that the mortality in either has been reduced; a circumstance which in itself militates very strongly both against the prophylactic and the remedial efficacy of belladonna.* But let facts like the following be, moreover, taken into consideration.

During the prevalence of scarlatina in Edinburgh and its vicinity, the writer of this article was requested to visit a young gentleman of twelve years of age, a boarder in an educational establishment at a little distance to the west of the city.† He was found to be labouring under well-marked scarlet fever, the characteristic eruption of which had made its appearance the day previously. In the room in which this boy lay there were other eight boys, only two of whom had suffered from the disease; and of sixty-five boys who lived under the same roof, there were thirty-eight who were in similar circumstances. The sick boy was immediately removed to the hospital at a little distance, the room he had occupied was well aired, and the bed-clothes removed from his bed; but, with these exceptions, no other means were adopted; the eight boys continued to tenant the same room, and no other case of the disease occurred.

A few months before this occurrence, the writer visited a young lady, one of a family of eight, whose ages were from six to twenty-four, occupying a comfortable, but neither very large nor very well-ventilated house, in the new town of Edinburgh. She passed through a severe attack of scarlet fever, having in particular very severe cynanche, and afterwards very profuse desquamation. None of her sisters or brothers, nor her mother, who nursed her, nor any of the domestics, contracted the disease. In the same street—not a very large one—there were, at the same time, at least two other houses in which the disease existed. These are not singular instances, nor are they recorded here in that belief; on the contrary, we know that the experience of every practitioner could amplify such a catalogue. But from such cases we do learn not a little; and this in particular, that the disease we have to deal with is a very fickle one, and that at times, in circumstances in which we should feel disposed to look with certainty to its diffusion and spreading, it will, why or wherefore we cannot tell, pleasingly disappoint us. Had belladonna been employed in either of these two instances, or in any of the numerous similar ones which have occurred, we do not doubt the exemption of the thirty-eight boys in the former, and of the eight individuals (exclusive of domestics, who were comparatively little exposed) in the latter example, would have been attributed to its prophylactic action.

As regards the "Edinburgh experiments," we attend to them here for this reason particularly, in order to notice a remark of Dr. J. D. Gillespie,

* The total number of deaths in England and Wales from scarlatina alone, during 1847, was 19,816; and in London, during 1848, out of a total mortality from all causes of 57,628, there died 4756 of this disease. It may be said that belladonna was only very limited in its employment; but so far as we have been able to learn, there are very few medical men who have not, at one time or other, employed it in their practice; it follows, that they have abandoned it on account of its inutility, or, as is the case with several, on account of its injurious consequences.

† At the same time, cases of scarlet fever had been known to have occurred in one of the houses nearest the place of this boy's residence; and but a few days before the attack we are presently to notice, the writer had been consulted in regard to the adoption of any measures which it might be expedient to put in force, seeing the disease, for which all directors or governors of such institutions stand in great awe, was then visibly within a few hundred yards.

who observed and described an epidemic of scarlatina which prevailed in Donaldson's Hospital.* Dr. Gillespie did not employ belladonna, because "had belladonna been administered, the experiment would not have been decisive without allowing the healthy children to mingle freely with the infected." This Dr. Gillespie did not deem warrantable, as very great facilities were afforded for keeping the children separated. Fifty-two children of a hundred who had not previously had the disease, took scarlet fever. Isolation of the sound from the sick, and removal of the sick from the part of the hospital occupied by the sound children, was, under Dr. Gillespie's judicious management, effective, to the extent of preserving 48 of the 100 children.

In criticising the accounts of the Edinburgh experiments, and contrasting, in particular, the experience of Dr. Gillespie in Donaldson's Hospital, with that of Mr. Bell in George Watson's, Dr. Henderson, while assuming that some of the boys in the latter institution were protected by the belladonna, speciously endeavours to account for (what appears to us) the entire failure of the drug in Mr. Bell's hands, by urging the greater liability of the boys to contract the disease on account of the amount of belladonna taken. This argument, though ingenious, is most fallacious. We shall not go back to the Hahnemannic view of the action of belladonna in scarlatina, further than to point out this fact, and it is a very striking one, that though Mr. Bell's care and attention, and evident determination to let the experiments in his hands have fair play, led him, "*in a few days*," owing to the dilatation of the pupil and impaired vision, to lessen the amount of belladonna the boys took; yet he never in any instance for months noticed either the *sore throat*, or the *rash over the skin of the body*, which Hahnemann described, and which he asserts led him to recognise in belladonna at once the prophylactic against, and the remedy in, scarlatina. Will it be contended that the small dose produces these symptoms, and the larger those which Mr. Bell has so faithfully described? If so, we can only add, that after repeated attempts we have failed to produce any rash by the employment of very small doses of belladonna.

Dr. Henderson makes no objection to the experience of Dr. Newbigging, in John Watson's Hospital, yet his boys received *larger* doses than Mr. Bell's, for he gave the extract in the proportion of *one-sixth to one-fourth* of a grain twice a-day, and *never* diminished it, continuing its use for five weeks. Mr. Bell began with *a fifth*, and finding, in a few days, that dose to be too large, he diminished it. Seeing that the injurious effects which led to the diminution of the dose by Mr. Bell were discernible in a few days, and that Dr. Newbigging continued the employment of the belladonna in some cases, in even larger doses than Mr. Bell had ever administered, we do think that a fairer statement of the case may be put than the one by Dr. Henderson. If *large* doses of the belladonna are to be regarded at once as exposing to the contagion of scarlatina, and as freeing from that contagion, there must be an end to all argument, for such a proposition tends in no small degree to the *reductio ad absurdum*.

Now, we do not mean to say, that Dr. Henderson wishes this to be believed as his opinion, yet his words undoubtedly admit of this interpretation: for when he consigns Mr. Bell's cases to the ready action of the scarlatina poison, owing to the *largeness* of the doses of belladonna

* Monthly Journal, 1853.

which they have consumed, and attributes the freedom from infection which Dr. Newbigging's enjoyed to their having had the belladonna administered—he in reality says nothing less—for *during five weeks* Dr. Newbigging continued to dose the children at John Watson's Hospital with *a fourth, a fifth, and a sixth* part of a grain, while the second of these was found by Mr. Bell, in the course of *a few days*, to be too large. It will not do to say that the continuance of the drug in Dr. Newbigging's experience for a few more weeks might have caused the children to take the disease; for, most assuredly, if such effects as Mr. Bell has described were produced with smaller doses than Dr. Newbigging for the most part employed, in the course of a few days, it is only reasonable to conclude that their continuance for a period far short of five weeks, would have produced all those effects upon the boys which Dr. Henderson imagines caused Mr. Bell's boys to fall an easy prey to the contagion. Dr. Henderson, determined to leave no stone unturned, having already, in regard to other experiments, suggested that some other drug than belladonna was used, conjectures that the extract of belladonna used by Dr. Newbigging was not so strong as that used by Mr. Bell. We venture, however, to remark that just on account of the variation in the strength of the extracts of belladonna, both gentlemen would satisfy themselves of the potency of the specimens they obtained.

The very accuracy which attended Mr. Bell's experiments, the evident care and attention he paid to all the particulars in connexion with them, makes his experience one of peculiar value; and we have little hesitation in saying that his "excellent" paper will continue to be regarded alike an authority condemnatory of the so-called prophylactic action of belladonna, and on the general treatment of the disease. It is our opinion that experience has altogether failed to recommend the employment of belladonna, and that now we should be prepared to abandon the practice, as not only insufficient but absurd.

We sum up our disbelief in the prophylactic action of belladonna on account of the following reasons:

1. Numerous facts attest its want of success.
2. All those facts which apparently testify in its favour admit of other and ready explanations.
3. These explanations are, in themselves, perfectly satisfactory and philosophical.

In conclusion: We have thus seen that it is impossible to accept the facts which have been advanced (with as strict a regard to impartiality as possible) as establishing the prophylactic action of belladonna; for though, at first sight, not a few of them seem to give countenance to that view, these do not so in reality, and very many directly oppose it. It may be that a prophylactic against scarlatina exists, but, assuredly, it yet remains to be discovered; meantime, our knowledge of what affords the best protection against that disease cannot be said to have advanced far beyond what was known to Frank, in whose words, equally truthful now as when written, we shall not inappropriately close: "Salus igitur in sola fuga contagii quæri debet, cui scopo regulæ adversus febres contagiosas jam traditæ, præcipue vero cura severa scholarum et ambulacrorum publicorum infantilium inserviunt."

J. Warburton Begbie.

Review VIII.

1. *Lehrbuch der Physiologischen Chemie.* Von Professor Dr. C. G. Lehmann. Dritter Band, 1852.

A Compendium of Physiological Chemistry. By Professor Lehmann. *The Chapter on Digestion.*

2. *Handbuch der Physiologischen Chemie.* Von Professor Dr. C. G. Lehmann.—*Leipzig*, 1854. 8vo, pp. 334.

A Manual of Physiological Chemistry. By Professor Lehmann.

3. *The Bakerian Lecture, on Osmotic Force: delivered at the Royal Society.* By Professor Graham, F.R.S., Corresponding Member of the Institute of France. ('Medical Times and Gazette.')

In the number of this Journal for July, 1853, we gave a succinct account, drawn from the most recent sources, of the digestive fluids in their physiologico-chemical relations; and more lately (p. 144) we have brought before our readers all that is contained, on the subject of respiration, in the admirable work of Professor Lehmann, which is at present being translated into half the languages of Europe. We purpose now to redeem a promise made on the former of these occasions, and to consider the important subject of absorption. Before entering upon it, however, we may briefly allude to the 'Handbuch der Physiologischen Chemie,' just published by the same distinguished chemist.

In this, which is, with some unimportant changes in the sub-arrangement, an abstract of his larger work, the author has, he informs us, endeavoured to put together, as concisely as possible, the positive facts which may, for the present, be considered as the sure possession of physiological chemistry, and to bring forward only those conclusions which, according to our present physical views, carry the stamp of relative truth. Professor Lehmann laments that, as yet, but few facts have been so fully established as to be undisputed, and that there are few points with which more or less of doubt is not connected. This, he feels, must increase the difficulty, always great, of giving a full account of a subject in a limited space, and where it is an object to avoid discussions, and the weighing of evidence. The author has, however, succeeded in overcoming this difficulty, for we must express our unhesitating opinion that he has, in the volume in question, produced a most excellent and useful abridgment of his great system of physiological chemistry; it is unnecessary to add, that an abridgment compiled by the author of a work must be both more trustworthy and more valuable than if drawn up by other hands.

It is right that we should, also, in this place, give a short account of Professor Graham's views on the osmotic force, for although we cannot at present see how far his discoveries will affect the subject of absorption, there is no doubt they must eventually do so, to a great extent. We abstract the following from the lecture, the title of which we have given above:

"The term 'osmotic force' is applied to the power by which liquids are impelled through moist membrane and other porous septa, in experiments of endosmose and exosmose. Diffusion and capillarity are shown to be insufficient to account for it.

"The nature and *modus operandi* of the chemical action producing osmose remains still very obscure. Salts and other substances, capable of determining a large osmose, are all chemically active substances, while the great mass of neutral organic substances, and perfectly neutral monobasic salts of the metals, such as chloride of sodium, possess only a low degree of action, or are wholly inert. The active substances are also, relatively, most efficient in small proportions. The chemical action must be different on the substance of the membrane, at its inner and outer surfaces, to induce osmose; and according to the hypothetic view, which accords best with the phenomenon, the action on the two sides is not unequal in degree only, but also different in kind. It appears as an alkaline action on the albuminous substance of the membrane at the inner surface, and as an acid action on the albumen at the outer surface.

"The most general empirical conclusion that can be drawn is, that the water always accumulates on the alkaline or basic side of the membrane. Hence, with an alkaline salt—such as carbonate, or phosphate of soda—in the osmometer, and water outside, the flow is inwards; but with an acid in the osmometer, on the contrary, the flow is outwards, or there is negative osmose, the liquid then falling in the tube."

A table is given exhibiting the osmose of substances of all classes.

"It may appear to some, that the chemical character which has been assigned to osmose takes away from the physiological interest of the subject, in so far as the decomposition of the membrane may appear to be incompatible with vital conditions, and that osmotic movements must therefore be confined to dead matter. But such apprehensions are, it is believed, groundless, or, at all events, premature. All parts of living structures are allowed to be in a state of incessant change of decomposition and renewal.

"The decomposition occurring in a living membrane, while effecting osmotic propulsion, may possibly, therefore, be of a reparable kind.

"In other respects, chemical osmose appears to be an agency particularly adapted to take part in the animal economy. It is seen that osmose is peculiarly excited by dilute saline solutions, such as the animal juices really are, and that the alkaline or acid property which these juices always possess is another most favourable condition for their action on membrane. The natural excitation of osmose in the substance of the membranes or cell-walls dividing such solutions seems, therefore, almost inevitable. In osmose there is, further, a remarkably direct substitution of one of the great forces of nature, by its equivalent in another force,—the conversion, as it may be said, of chemical affinity into mechanical power. Now, what is more wanted in the theory of animal functions than a mechanism for obtaining motive power from chemical decomposition as it occurs in the tissues? In minute microscopic cells, the osmotic movements, being entirely dependent upon extent of surface, may attain the highest conceivable velocity.

"May it not be hoped, therefore, to find in the osmotic injection of fluids the deficient link which certainly intervenes between muscular movement and chemical decomposition?"

We shall now proceed to the consideration of the more immediate subject of this article, and in doing so, will bring before our readers a full abstract of Professor Lehmann's chapter on the function of absorption. This chapter, it can scarcely be necessary to observe, was written before the promulgation of the views put forward in the lecture by Professor Graham, and just now quoted. As the 'Handbuch' bears a considerably later date than the chapter in question, we shall, in passing through the latter, note any new views or changes of opinion which may be expressed in the former.

Professor Lehmann considers that, notwithstanding the labours of the most distinguished investigators, as Poisson, Magnus, Brücke, Liebig, Jolly, Ludwig, &c., we are still without any satisfactory theory of endosmose. The establishment of such a theory is the problem, to the solution of which our attention should be next directed, for we shall be unable properly to comprehend the mechanism of absorption until we have more accurately ascertained the physical conditions of endosmotic effects, and compared them with the circumstances which obtain in the living body. To enable us to do this, it will be necessary to make out the laws of the diffusion of fluids in the manner proposed by Graham, and thoroughly to investigate the influence of the various kinds of porous septa or diffused fluids—i. e., the relation of diffusion to endosmose, and all the circumstances influencing the latter. Then only will it be possible to prove or disprove the co-operation of vital powers in absorption.

In the 'Handbuch,' the author having pointed out, that in analogous bodies the degree of capability of being absorbed will always coincide with certain closely allied physical and chemical properties, as solubility, the density of the solution, the diffusibility or endosmotic equivalent, observes, in speaking of the absorption of matters in the intestine, that "*absorption is nothing but a function of the mechanical conditions just now mentioned.*" (p. 260.) He also shows that absorption through the bloodvessels is favoured by the following circumstances: first, that the solutions of matters existing in the intestine are extremely dilute, while the blood is the most concentrated fluid of the body; hence the endosmose of these very dilute solutions is greatly promoted. This state of things is soon disturbed by the injection of large quantities of water, unless the blood be kept at its due degree of concentration by the abundant removal of the water from the blood, through the lungs, skin, or kidneys.

A second circumstance which favours the transudation from the intestine into the blood, is the constant motion of the latter in the capillaries, by which the portion which has been diluted by the watery fluid of the intestines is immediately replaced by concentrated blood.

A third is the normal acid reaction of great part of the contents of the intestine, while the fluids destined to take up the intestinal solution have a strongly alkaline reaction. "But we know," he adds, "that free acid promotes exosmose, while free alkali impedes it." (p. 262.)

The author divides the objects of digestion into, first, those which are brought into the alimentary canal in a state of solution, and immediately becoming diffused, enter into the general mass of the fluids of the body; secondly, those which are rendered soluble by the action of the digestive fluids, and so become likewise more or less diffusible; and finally, those which, dissolved or undissolved, must first be changed by certain digestive agents, and then, even though soluble, do not yield to a simple diffusion, but are introduced into the blood and the rest of the body in peculiar modes, being, before their arrival in the blood, subjected to some, though it may be trifling, changes.

The substances which undergo no essential change in the intestinal canal from the action of the digestive fluids, are particularly adapted for immediate absorption by the bloodvessels, yet it is not clearly ascertained what relation exists between these two qualities. It is not the saline

nature alone which makes the alkaline salts so easy of absorption, for many other salts are not taken up by the capillary bloodvessels; and, on the other hand, urea, alcohol, and certain poisons pass as easily, and perhaps more quickly, into the fluids of the body than many of those salts; neither is it merely the degree of solubility of a substance, but it is the union of many qualities, which confers both a capability of being absorbed, and a power of resisting the action of the digestive fluids. Since there are many poisons which the system quickly takes up from the intestinal canal, and others which are not so absorbed, we cannot expect to find the reason of these facts in an instinct of the absorbent organ, but in definite fundamental principles of the substances. On this account the author thinks it advisable to group the objects of digestion, not according to their nutritive powers, but, without reference to their useful or injurious properties, to divide them, as we have just stated, according to their digestibility—i.e., according to their greater or less capacity for being absorbed.

The author gives the following enumeration of the substances which reach the circulation, not through the lymphatics, but directly through the capillary bloodvessels:—all the neutral salts of the alkalies, the acids of which have not a greater affinity for other bodies to be met with in the intestinal contents; among these are the chlorides of sodium and potassium, the iodides and bromides of potassium; the phosphates, sulphates, chlorates, nitrates, borates, and arseniates of the alkalies; yellow ferrocyanide of potassium; the compound of rhodium and potassium (*Rhodankalium*); and the compounds of alkalies, with such organic acids as do not contain nitrogen. A second group of bodies, which are chiefly absorbed by the intestinal capillaries, are the acids, both mineral and organic. A third group contains alcohol, ether, wood-spirit, fusel oil.* A fourth, several volatile oils, both free from oxygen, and containing oxygen and sulphur (camphor, oil of radishes, oil of assafœtida, &c.); to this class belong also the empyreumatic and natural odoriferous matters, as musk, and the constituents of the animal oil of Dippel, &c. A fifth, several alkaloids, both fixed and volatile, for example, strychnia, brucia, morphia, thein, nicotin. Lastly, some pigments should be enumerated, which are not to be found in the chyle, but in the urine: for example, the colouring matters of alkanet, gamboge, bilberries, black cherries, rhubarb, logwood, madder, litmus, cochineal, sap green, and tincture of indigo.

In so great a variety as is presented by the substances above enumerated, it would be difficult, if not impossible, to find a common aggregate of properties to which their capability of absorption through the blood-vessels might be referred; but certain other bodies, which far exceed them in solubility, for example, do not, in direct experiments, show the least tendency to pass into the blood through the capillaries, while they are very easily taken up by the lymphatics, or, notwithstanding their great solubility, traverse the entire intestinal tract unabsorbed; thus gum, the colouring matter of turmeric, &c., which are extremely soluble, are neither taken up from the intestines by the bloodvessels nor by the lymphatics. The curara poison, which is probably identical with the wourali, and the

* Schlossberger: Arch für Physiol. Med., Band ix. § 267—269.

poison of serpents, appear to belong to the latter class of bodies; we might think this a wise provision of nature, were it not that gum and turmeric pigment, which are comparatively harmless, are denied access to the chyle and capillaries equally with the poison of serpents, which seldom reaches the stomach: while no obstacle exists to the absorption of other poisons which are seldom received in wounds, but usually reach the intestine. It is clear that only soluble matters are capable of absorption, but the solubility of those quoted above is so variable that we cannot, by it alone, explain their capability of being absorbed by the capillary blood-vessels. The diffusibility of most of the substances, and their endosmotic equivalent, which is incontestably connected with it, have as yet unfortunately been too little investigated to refer their facility of absorption to these principles; but it is probable that this facility depends on their diffusibility, their volatility, and a certain simplicity of composition, approaching to a binary constitution; accordingly, those soluble matters which belong to none of the above groups, as albumen, emulsion, gum, and even sugar, have resisted all the attempts of chemists to account for their composition by reference to the usual laws of chemical affinity or polarity. In the 'Handbuch' it is stated that the substances which have been ascertained to be very diffusible, are absorbed through the blood-capillaries; while those the power of diffusion of which has been proved to be small, are taken up by the lymphatics. (p. 264.)

Professor Lehmann wishes not to be misunderstood in all this, as believing the process going on in the living body to be entirely physical; he is far from attempting to establish so purely mechanical a view; but he considers that the simplicity of physical principles affords a better foundation for our hypotheses, and a surer direction to our future investigations, than we should have, did we without earnest and deep reflection lightly throw ourselves into the arms of transcendental reasonings.

The substances we have enumerated do not, properly speaking, become the objects of digestion, as they pass into the circulation from the intestinal canal, in the same state in which they reached the latter. The compounds which some of them form within the body with acids, need scarcely be mentioned, as the acids do not effect any essential change in them.

Passing, then, to the actual objects of digestion, we first meet a group of bodies "which," observes Professor Lehmann, "have received the irrational name of carbo-hydrates; we know that among these cellulose, the several varieties of gum, starch, inulin, lichenin, and the true sugars have been enumerated."

Cellulose, or vegetable cellular matter, belongs to those bodies which resist the action of all the digestive fluids and other solvents; accordingly we find all vegetable tissues, which consist essentially of this substance, unchanged in the excrement of herbivorous and omnivorous animals. Cellulose has been proved by Mitscherlich to be perfectly isomeric with starch $= C_{12}H_{10}O_{10}$.* Some have supposed that the digestive fluids of the beaver may be capable of changing and dissolving it, and in fact, the organs whose secretions serve for the metamorphosis of the carbo-hydrates —viz., the salivary glands and pancreas, are unusually large in this animal.

* Annalen der Chemie und Pharmacie, Band lxxv. § 305—314.

It is doubtful whether the large gland attached to the stomach, and peculiar to the beaver, may be connected with the digestion of cellulose. Schleiden and Mulder have shown that cellulose is converted, by treatment with the second or third hydrate of sulphuric acid, into a substance very similar to starch; and Mulder has proved that phosphoric acid, of the consistence of syrup, may replace the sulphuric acid. Still, acid as the contents of the stomach of the beaver are usually found to be, they must be too dilute to allow us to ascribe this action to them. Mitscherlich's investigations of the action of very dilute solutions of the alkalies on cellulose, render it more likely that the alkaline juices of the salivary glands, the pancreas, and the cæcal glands effect its conversion into starch, and its further change into sugar. This chemist, too, has shown that a peculiar ferment, formed by the putrefaction of potatoes, exists, capable of destroying the cells of cellulose without attacking the starch. Might such a ferment exist in the juices of the beaver, and, in conjunction with the contents of the lower part of the small and of the large intestine, which in this animal have a tolerably strong alkaline reaction, accomplish the digestion of this substance? Still we cannot regard the digestibility of cellulose, even by the beaver, as fully established, until more direct proofs can be adduced in support of it. In the 'Handbuch,' caterpillars are also mentioned as probably possessing the power of converting cellulose into sugar, and as having highly-developed salivary organs.

Notwithstanding its solubility, the reception of *gum* into the animal organism is still doubtful. Though it seldom occurs in the food even of herbivorous animals, its frequent therapeutic employment, and its peculiar chemical and physiological behaviour, demand attention. The results of experiment make it highly improbable that even a small portion of gum is changed in digestion into sugar. All attempts to discover it in the chyle, blood, or urine have failed; but it is largely found in the excrements of animals fed on it:—thus, of 50 grains given to a duck, 46 were recovered from the excrement passed in the course of nine hours. From these and other experiments, it is evident that if this substance be at all capable of being absorbed, it is only very slowly, and in very small quantity, that it can pass into the circulation.*

Has anything been ascertained as to the diffusion or transudation of gum, which may account for the foregoing facts? According to Graham, its diffusibility is one-half less than that of sugar from starch, and four or five times less than that of chloride of sodium, but four times higher than that of albumen. Jolly found the endosmotic equivalent of gum to be much greater than that of sugar. The simplest endosmotic experiment will prove that animal membrane is not impermeable to gum; it remains to show what the mechanical conditions are which allow the passage of but so very little gum from the digestive tube into the blood. In a word, much remains to be done before we can pronounce a decided opinion on the behaviour of this substance in the intestinal canal, or venture to

* This is at variance with certain facts which appear well authenticated. The late Dr. Pereira quotes an instance in which a thousand persons supported themselves for two months on the gum which they were carrying as merchandise; and six or eight ounces for an adult are said to be sufficient to sustain life. Elements of Materia Medica and Therapeutics, by Jonathan Pereira, M.D., second edition, 1842, p. 49.

assume the interference of vital powers in resisting its absorption. The use of mucilaginous mixtures in acute diseases, if any, is evidently only negative.

"It is well known that *starch* is the most important of all the carbo-hydrates, when considered as objects of digestion; we know that it belongs to those bodies which, in order to be absorbed, require to undergo a previous change; that the conversion of starch proceeds to the formation of dextrine and sugar, but that lactic acid is generated only to a more limited extent; and, lastly, that the saliva and pancreatic juice are the means by which this conversion of the atoms of starch is brought about."

The facts brought forward by Lehmann in this portion of his work, in reference to the action of these fluids upon starch, are drawn from Bidder and Schmidt, and have been fully considered in our previous article; we need not, therefore, dwell upon them at present.

In addition to what we have there noticed, we may, however, observe, that a very great proportion, at least of raw starch (i. e., starch whose corpuscles have not been destroyed by boiling or other operations before their reception in the intestinal canal), remains unchanged as far as the rectum, and is very commonly found in the excrements. Starch-paste, when chewed, acquired a decidedly sweet taste in the course of two minutes. Unboiled starch did not acquire a sweet taste, even when subjected to mastication for ten minutes.

The behaviour of the digestive fluids to *inulin* is the same as to starch.

Sugar next engages the author's attention, and of its varieties he gives precedence to *glucose*, on account of its frequent occurrence in vegetable food, and of its importance as the most usual and normal product of the conversion of our most important unazotized article of nutriment, starch. Two questions present themselves in connexion with this substance—first, Does it become absorbed in an unchanged state, or does it undergo any previous change? This we have already answered in the author's words.* Secondly, Through what organs is glucose absorbed from the intestinal canal?

It is usually supposed that sugar introduced into, or formed in, the digestive tube, is simply and quickly absorbed, unchanged, by the capillary blood-vessels; a view which Professor Lehmann considers to be far from proved. He fed horses for three days on starch, one half of which was boiled, and the other half raw, mixed with one-twelfth of its weight of rye-bran; in addition, he gave each horse one kilogramme (about 2·2lbs.) of sugar in about twenty-four hours; on the third day, the amount of starch passed in the excrement of twenty-four hours was ascertained, and varied in the different horses from about a fourth to about a sixth of what had been consumed during the last twenty-four hours. In an hour and a half after the last feed, the horses were killed; the contents of the intestinal canal were then examined, and the chyle and the blood of the vena portæ were subjected to a careful analysis, in reference to their quantity of sugar.

The blood of the vena portæ was obtained by making a small opening in the abdominal parietes, and tying the vessel at its entrance into the liver, previously to opening the vein. This precaution was necessary, to prevent

* Vol. xii. p. 195.

a reflux of blood from the hepatic veins, which has misled some observers as to the presence of sugar in the vena portæ, as well as with reference to the nature of the blood of this vessel. Professor Lehmann did not, in any instance, discover either sugar or a trace of dextrine in it. He does not, however, infer from these simple experiments, that no sugar at all is, as such, taken up by the intestinal capillaries, but feels himself justified in maintaining that only a very small quantity of sugar can reach the vena portæ from the intestine.

Direct experiments proved that the absorption of sugar from the intestines does not go on so quickly as has been supposed. One or two grammes of sugar of starch were injected into the pharynx of rabbits, fed immediately before, and also after, with solid nourishment; the animals were killed in half an hour, one hour, and two hours subsequently; in every instance sugar was found in the stomach, duodenum, and jejunum, the contents of the two latter were strongly acid. Sugar (glucose) in considerable quantity was found in the stomach and duodenum of rabbits fed for some days previously solely on beet-root or on carrots.

Sugar, then, appears not to be as quickly absorbed by the intestinal bloodvessels as many other very soluble substances: the question remains, is it taken up in proportionally greater quantity, or perhaps solely, by the lymphatics? Professor Lehmann's experiments quite refute such a view. From the chyle of one of the horses alluded to, he obtained an infinitesimal quantity of sugar, in that of the other two he could scarcely discover a trace. His conclusion on this subject is, that "it cannot be denied that sugar is absorbed by the lymphatics, but it is certain that the amount which enters these vessels is a very small fraction of the quantity formed in the intestine from starch." Still, we cannot show that all the sugar in the intestine is further changed; all that we are as yet able to prove is, that a considerable quantity of starch quickly passes in the intestine through the transition stages of dextrine and sugar, to be converted into lactic acid; and that cane sugar, as well as glucose, very soon undergoes a similar metamorphosis. Bidder and Schmidt having opened the abdomen of a cat, pressed the contents out of a portion of the intestine, and subsequently opened it, introduced very thick paste of starch, tied the loop of intestine in two places, about three inches apart, and in three hours after examined the contents, which they found to consist of a fluid mass containing much sugar, very little starch, and having a strongly acid reaction. There can be no doubt that during normal digestion, a certain quantity of starch and sugar is changed into lactic acid. The foregoing experiment shows that even when the saliva, gastric juice, pancreatic juice, and bile are wholly excluded, the intestinal juice alone is competent to change starch into sugar, and sugar into lactic acid; indeed, it seems to exceed all these fluids in the rapidity with which it causes the formation of lactic acid.

The 'Handbuch' contains a few additional observations on this subject, and in them we may find the explanation of some of the foregoing facts.

Carefully-instituted experiments on animals have led to the following results, in reference to the absorption of sugar:

It has been proved that, when solutions of sugar are enclosed in tied loops of intestine in living animals, the quantity of sugar absorbed in a

given time is quite independent of the length of the loop or of the square extent of the absorbing surface; it is only when the tied loop, containing the concentrated solution of sugar, is so short that it cannot take up an amount of water corresponding to the endosmotic equivalent of the sugar, that an exception to this rule occurs.

Secondly, the absorption of the solution of sugar is in the *direct* ratio of its concentration. Entirely in unison with the laws of endosmose, do we see the loop of intestine, containing a concentrated solution of sugar, become distended in consequence of the absorption of water; a quantity of sugar, corresponding to the amount of water received by the intestine, enters the blood, until all the sugar has disappeared from the loop.

From the endosmotic law, it is evident why the extent of the intestinal loop is, if it be not less than a certain length, without influence on the absorption of sugar. If the loop be large enough to admit of the entrance of the equivalent quantity of water, only the corresponding amount of sugar can leave it, be the loop never so large. As the quantity of water which enters is dependent on the amount of sugar in the injected solution, the absorption must remain exactly the same, even in loops of the most different sizes, if the concentration of the solutions be equal.

In the results of these experiments, we find the explanation of the facts we have quoted, of the slow absorption of sugar from the intestine where it, under normal circumstances, occurs for the most part in very dilute solution, its rapid spreading over the entire small intestine, &c.

Hence it is difficult, with precision, to answer the question—important as it is in the physiology of the quantitative metamorphosis of tissue—How much sugar can, in a given time, be absorbed by an animal? as the result of the experiment will always depend on the degree of concentration of the solution introduced into the intestine. If this be very dilute, absorption will proceed slowly, as under normal circumstances; if it be concentrated, a very great quantity of water will be withdrawn from the blood, the intestine will become so filled with watery fluid as to distend the abdomen, and intense dyspnœa, and often death, *will* be produced.

It is likely that the gastric juice is the agent in digestion which effects the metamorphosis of *cane-sugar* into glucose. In the 'Handbuch,' this effect is, however, attributed rather to the action of the pancreatic juice, or the ingredients of the intestinal contents.

Sugar of milk behaves, in the intestinal canal, like glucose, passes very quickly through the small intestines, can be traced, in about an hour after being taken into the mouth, to the cæcum, and, like glucose and cane-sugar, leaves an intensely acid reaction in the jejunum and ileum, which continues for three or four hours after the sugar has been swallowed.

Vegetable mucus passes, for the most part, unchanged with the excrement (Frerichs), as do *pectin* and its derivatives.

No class of aliment has at all times presented so many difficulties to the physiologist as the *fats;* and even in the present day, we cannot flatter ourselves that we perfectly understand the process of their digestion. Our experience of some of their physical and chemical properties renders it difficult to explain the mode of their reception into the blood; since they are absolutely insoluble in water and watery solutions, and a watery fluid everywhere permeates the tissues of the body, we cannot admit their

diffusion, in the ordinary acceptation of the word; while their decomposition requires either more powerful agents than are usually met with in the intestinal canal, or a longer time than they generally remain in it. The saliva and gastric juice exercise no influence on the mechanical or chemical condition of fat, and all investigators admit that its digestion does not commence until it has reached the duodenum. Here, but still more in the lower portions of the small intestine, the globules of fat become smaller, the fat becomes more finely divided, and the chyme more like an emulsion. It is, therefore, with the aid of the microscope that we can trace the progress of its digestion.

Bidder and Schmidt have very clearly proved that Bernard was in error when he stated, that the absorption of fats was entirely due to the action of the pancreatic juice. Still, though the opinion of the importance which the latter observer entertained with respect to this fluid appears to have been refuted, certain facts discovered by him cannot be wholly laid aside. Thus, it cannot be denied that no animal fluid, when shaken for a short time with fat, gives so perfect an emulsion as the pancreatic juice; no animal fluid retains fat so long in a state of emulsive suspension: in reference to this property, the bile, for example, is not to be compared with it. The pancreatic juice, further, contains the only body, found in chemistry, which is capable of resolving with such rapidity the neutral fats into glycerine and fatty acids. Whosoever has brought fresh viscid pancreatic juice in contact with fat, cannot suppress the thought that it must essentially contribute to the digestion of this substance. But the experiments of Bidder and Schmidt will be found so conclusive, that this idea must, at least for the present, be given up. Lassaigne and Colin found the pancreatic juice of the calf and the horse to be very thin, and incapable of forming an emulsion with oil, while that of other herbivorous animals possessed this property in a high degree.

Bidder and Schmidt have shown by experiments on dogs, in which artificial fistulæ of the gall-bladder were produced, the ductus choledochus having been previously tied, that the bile which passes into the intestine is without any influence on the digestion of albuminous matters and of starch, but that it has a great effect in determining the quantity of fat to be retained in the body and applied to the purposes of life, 2½ times less of the latter principle being, in the most favourable case, absorbed when the supply of bile is cut off than when it is allowed uninterrupted access to the intestine.

"The participation, then, of bile," observes Lehmann, "in the digestion of fat, is placed beyond all doubt, although it can by no means be denied that a small portion is absorbed without the co-operation of this fluid."

Brodie, as well as Tiedemann and Gmelin, thought they had proved, that after tying the ductus communis choledochus, the lacteals contained, notwithstanding the use of a diet abounding in fat, a colourless transparent fluid; while Magendie, and recently Lenz, observed their contents to be milkwhite under similar circumstances. But it has been shown, that even when bile is wholly excluded from the intestine, a small portion of fat is absorbed, which would be sufficient to give the chyle a milky appearance. Schmidt, however, proved, by quantitative analysis, that chyle taken from the thoracic duct of a healthy dog, fed with beef eight

hours before death, contained many times more fat than that obtained from dogs furnished with artificial biliary fistulæ, although the other ingredients of the chyle varied but little in the several specimens; experiments which fully confirm the statement, that the bile essentially contributes to the resorption of fat. We have in our twelfth volume alluded to the fact, that bile seems to render the intestinal mucous membrane more permeable for fats.

It is clear, then, that fat in digestion is taken up principally by the lacteals; but Chr. Fr. Schmidt's and the author's observations, show that the capillary bloodvessels take up a small portion, as is proved by the increase of fat in the contents of the portal vein some hours after feeding. This fat does not, however, pass directly into the capillaries, but, in common with the salts and all other matters which do not enter the lacteals, it has previously to traverse several series of cells.

We now pass to the group of bodies which must, before being absorbed, undergo an essential change in the intestinal canal. To this class belong not only the albuminous matters, but also their remote derivatives, as, for example, many gelatinous substances, and in addition to these, a number of less well-known matters, as synaptase and diastase, the poison of serpents, curarin, &c. The albuminous matters are not merely dissolved, but are changed into substances, which, although similar in their elementary composition to those from which they are derived, yet, in their physical and many chemical properties, differ essentially from them. The author proposed the name Peptones, for albumen, fibrin, casein, &c. changed by the gastric juice, believing that it is these peptones which undergo absorption, to be subsequently metamorphosed in the lymphatics into the well-known coagulable albuminous matters.

In speaking of the gastric juice, we have stated the discordant results of Lehmann's and Bidder and Schmidt's experiments, as to the proportion of albumen which that fluid is capable of dissolving. Schmidt has shown that gastric juice free from saliva is a more powerful solvent of albuminous matters than that which is mixed with the latter secretion; doubtless it loses some of its influence on these bodies by the saturation of a portion of its acid by the alkaline saliva. He has likewise proved that the addition of bile wholly removes its solvent action, even when the mixture still shows a decidedly acid reaction; hence, if undigested albuminates reach the duodenum, the gastric juice will have lost all power over them. If there be acid reaction in the duodenum, it proceeds not from free hydrochloric acid, but from biliary acids separated by it, which very soon become insoluble, or are absorbed; so that usually after the use of meat, no acid reaction is to be found in the jejunum. But although the gastric juice is by no means sufficient to digest completely all the protein taken into the stomach of carnivorous animals, and although the bile and pancreatic juice have no effect on the protein compounds, the albuminous substances which leave the stomach undigested are seldom found in the excrement; they must therefore be dissolved, and rendered capable of being absorbed by the action of the intestinal juice. And in fact, Bidder and Schmidt have, in the clearest manner, demonstrated that this fluid possesses the power of digesting coagulated and insoluble protein compounds; and it is remarkable that the bile and pancreatic secretion, which deprive

the gastric juice of the power of digesting the albuminates, do not even diminish this property in the intestinal fluid.

In the large intestine, however, the protein compounds can suffer but little change; not only is the amount of fluid secreted in this part extremely small, but Lehmann's and Steinhauser's experiments show that albuminates introduced into the large intestine, or lower part of the ilium, are found almost unchanged in the excrement.

Besides the protein bodies and their immediate derivatives, many other substances may be classed in this fourth group, which do not possess the great physiological value of the albuminates, but have a similar behaviour in digestion. We have already spoken of the gelatinous tissues in this point of view. In addition, we might enumerate some poisons which, like the albuminates, are not capable of direct absorption by the blood-vessels, but are, in the first instance, so changed by the gastric and intestinal juices, that they are taken up by the lacteals, and reach the blood as harmless substances. Nearly allied to the protein bodies, without exactly belonging to them, are diastase and emulsin; and analogous to emulsin are many substances which have, it is true, been less accurately investigated, but which agree with one another in their amount of nitrogen, and in being insoluble in spirit, and soluble in water. Of these bodies, curarin has been, perhaps, the most closely examined; taken into the digestive tube, it gives rise to no morbid phenomenon, while, introduced into the blood, it causes almost instant death. Closely related to it, both in a toxicological and chemical point of view, are the poison of vipers, and those poisons emanating from contagious diseases, as hydrophobia, typhus, distemper among cattle, &c.; at least, Renault* has shown that the flesh of beasts affected with these and similar maladies, may be eaten with impunity by carnivorous and omnivorous animals; while it is well known, that the juice of such flesh introduced directly into the blood or into wounds, produces consequences most dangerous to life; proving that these matters are incapable, in their unchanged state, of being absorbed either by the bloodvessels or the lymphatics of the intestinal canal; and since most of them cannot be demonstrated in the solid excrements, it is evident that they must be so metamorphosed by the digestive fluids, that when they reach the blood, they can no longer exercise their poisonous qualities. While Lehmann cannot agree with Bernard, that animal membranes are wholly impenetrable to these bodies (emulsin, diastase, curarin, and the poison of vipers), he observes, that it is established that, like albumen, they have very weak endosmotic powers; and it is for this reason that the protein bodies, apparently so fitted for the purposes of nutrition, must be metamorphosed by the digestive fluids before they can be absorbed. Without the intervention of the gastric and intestinal juices, even soluble albumen and casein would be taken up from the digestive tube in far too small quantity to suffice for the support of the organism. In fact, the albuminates are absorbed in very small quantity; it is not until they become peptones that they are largely taken up.

We have as yet no idea of the mechanism of absorption through the lymphatics. All attempts to explain it have been directed to the

* Comptes Rendus, tom xxxiii. pp. 532—535.

mechanism of the motion of the fluids in these vessels, but not to the process of absorption itself. Even the discovery by Brücke of fibre-cells in the intestinal villi, which has been confirmed by Kölliker, while it explains the emptying of the commencement of the lymphatics, does not unravel the mystery of the mode in which these minutest ramifications of the lymphatics become filled. It is the capillary bloodvessels of the villi, rather than their lymphatics, which may be compared to the fibrillæ of the roots of vegetables. In the lymphatics there is no fluid so concentrated, or no soluble substance, to cause an attraction of the fluids from the intestine; indeed, the points of the lacteals do not float in the intestinal fluids; the latter must traverse several series of cells, and come in contact with the fine capillary vessels before they reach the actual lymphatic. It would appear as if only what is difficult or incapable of absorption by the bloodvessels is taken up by the lymphatics. But neither can we, by assuming a specific permeability of the membranes, solve the problem of absorption through the latter vessels.

In our previous article we mentioned the results of Bidder and Schmidt's experiments, as to the quantities of the different digestive fluids secreted by an adult man in twenty-four hours. Startling as it may appear, the juices flowing into the *intestinal canal in that space of time, amount to about the sixth part of the entire weight of the body*, and form a mass of fluid far greater than the quantity of blood which, according to the more recent and most trustworthy calculations is contained in the body of an adult. This is, however, sooner or later, almost completely received back again into the vessels, a constant flux and reflux of watery solutions, which cannot be unimportant, and must react on the processes of nutrition and metamorphosis of tissue proceeding from the blood.

Recent investigations have placed beyond all doubt the immediate dependence of certain secretions on special portions of the nervous system: thus, not the smallest quantity of saliva will be secreted without the influence of the nerves; and Ludwig and Rahn have, by rigid experiments upon rabbits, proved the direct effect of the nervous system upon the parotid, through the facial nerve, indirect through the third branch of the trigeminus (by exciting to masticatory movements), and its reflex through the glosso-pharyngeal nerve. That the secretion of the gastric juice is under nervous influence, is proved by the copious flow of this fluid excited by holding tempting food before a fasting dog; and the par vagum has been supposed to be the agent in this case, yet the division of both vagi has been found in some cases to interfere neither with the secretion of the gastric juice, nor with the motions of the stomach. But it is not credible that a nerve, which so abundantly supplies this organ with filaments, should influence neither its secretions nor its motions, especially as Bidder and Schmidt have shown that it has nothing to do with the production of the feeling of hunger. Volkmann has correctly traced these apparent anomalies to the anatomical relations of the nerve, for it expends the greater part of its cerebro-spinal filaments on the head and upper part of the neck, and the further it sinks down towards the diaphragm the more filaments it receives from the sympathetic; the abdominal vagus, then, is quite different from what it is when issuing from

the skull; it contains filaments which cannot be excited by its cervical portion, and the effect of which on the motions of the stomach cannot be interrupted by the division of the nerve in the neck. What is true of the motion of the stomach in this respect, may also be true of the secretion of the gastric juice. Thus it is clear, that further experiments, connected with great difficulties, are required in order to answer the question, on what nerve or combination of nerves the secretion of the gastric juice immediately depends?

Having considered the process of digestion in its several bearings, and especially the relations between the objects of digestion, the digestive agents, and resorption, the author proceeds to give some hints as to the digestibility of compound aliments, as distinguished from the alimentary principles already spoken of. This is, however, a subject beset with many difficulties; and, as the author does not place much confidence in the results of his experiments, we shall touch but lightly on them. It is one, he observes, which belongs more properly to the practical physician than to the chemist. Physiological chemistry can do little more than supply the physician with the fixed principles or scientific means by which he may more accurately appreciate well-observed practical facts, and on them commence the foundation of a dietetic system.

By the digestibility of an aliment we understand the ease with which the digestive fluids prepare it for absorption, or the shortness of time in which it undergoes absorption, and disappears from the intestinal tract. This is much influenced by the quantity of food ingested at a time. Soluble coagulable albumen is, according to the result of Lehmann's experiments, changed in the stomach; and is not, as Frerichs supposed, absorbed unaltered. The albumen of one egg given to a dog previously kept for twelve hours without food, had entirely disappeared from the stomach at the end of one hour, but when the albumen of eight or more eggs was given at a time to the same dog, the presence of coagulable matters in the stomach could be demonstrated after the lapse of three or four hours.

Fluid, finely-divided, and porous aliments, are more accessible to the digestive fluids, and must consequently be easier of digestion [*cæteris paribus*] than others which do not possess these properties in a similar degree.

Frerichs has demonstrated by experiments on living animals, that boiled fibrin dissolves much more slowly in the stomach than unboiled; and a similar result is obtained by treating both kinds of fibrin outside the system with natural or artificial gastric juice.

Soluble casein, as it occurs in milk, is, as is well known, very quickly coagulated in the stomach, and is then again, but gradually, dissolved or digested; consequently, it is probably the most difficult of digestion of the unboiled protein substances; it differs much, however, in this respect according to the looser or denser quality of the coagulum, for, as Elsässer states, the jelly-like coagulum of women's milk is much more quickly digested than that of cows' milk, which forms in the stomach a compact ball-like mass.

Gelatine is one of those bodies which most easily dissolve in the stomach. Tendons and cartilage are among the substances most difficult of digestion,

and are often found little altered in the excrement of carnivorous animals: the proper elastic tissue and elastic fibres entirely resist the action of the digestive fluids.

Chemically-prepared muscular fibre is, as appears by some experiments of the author's, very easy of digestion; in the coagulated state it is about equal to coagulated albumen and casein. Although this substance is perfectly identical in all kinds of flesh, and also in the smooth muscles, experience shows that the digestibility of the smooth and striated muscles, and even of the latter, from different animals, is extremely different. This is owing to the fact of the former wanting the dense though thin coating, so difficult of solution, in which the fasciculi of the latter are enclosed; and to their being enveloped merely in loose areolar tissue, which is easily permeated and dissolved by the digestive fluids. It was for this reason that Beaumont found tripe to disappear so quickly from the stomach (within an hour), and oysters to be digested more quickly than beef and other varieties of meat. Professor Lehmann goes on to say, that the flesh of young animals is, in consequence of its fasciculi being much thinner, and consequently exposing a greater surface to the action of the gastric juice, much easier of digestion than that of older animals. This is certainly contrary to the usually-received opinion, as experience seems to prove that lamb and veal, for example, are not so easily digested as mutton and beef. Fish is likely to prove difficult of digestion to many people in whom the powers of digestion are not over strong, because, when it is brought, in a state of fine division, into contact with fluids, it forms a solid homogeneous lump, on which the digestive juices can work but slowly from the surface. Neither can meat be classed with the easily digestible bodies, because the areolar tissue must be dissolved before the gastric juice can reach the muscular fibres.

The difference between the digestibility of raw and boiled or roast meat is not very considerable, the good effect of cooking, in loosing the areolar tissue and partially destroying the organic structure, is, to a certain extent, counteracted by the coagulation of the albumen contained in the juice of the meat. The author adds, that meat which has lain in vinegar is rendered more digestible by the loosing of its areolar tissue and muscular fibres, while its digestibility is greatly impaired by the process of smoking.

As the digestion of fat does not commence until it reaches the small intestine, this substance, if taken into the stomach in large quantity, acts injuriously by liquefying and enveloping other substances, which it thus renders inaccessible to the digestive fluids; it also, if detained in the stomach, becomes rancid, and forms volatile acids, which, in some way not clearly understood, materially injure digestion; but if used in smaller quantity, it is quickly absorbed, and even facilitates the digestion of many albuminous and amylaceous substances.

The chief nutritious principle contained in *vegetables* is starch, which, if not taken in quantity disproportionate to the amount of saliva, pancreatic juice, and intestinal juice, is easy of digestion. But the digestibility of vegetables generally depends on the nature of the cells which contain the starch and protein bodies; if these be covered with epidermis, nothing will be dissolved, as the epidermis of plants is perfectly impermeable to

the digestive fluids. Boiling is useful by loosing the intercellular substance of the parenchyma, and by bursting the outer layer surrounding the starch-corpuscles: but the most important part of the digestion of vegetables takes place in the small and also in the large intestine; and the enormous size of the cæcum in most herbivorous animals, shows that an essential part of the process must there take place.

To these remarks on digestion, we may add a few concluding observations on nutrition.

In estimating the nutritive power of food, we must consider two principal points—first, the proportion in which it contains the four grand elements of animal regeneration—viz., albuminous matters, fat, carbo-hydrates and salts; and, secondly, the circumstances under which the organism has more or less need of all or any of those elements, in order to maintain its integrity, or to produce certain powers of action. In this estimation, the condition of the elements belonging to the several groups is not to be lost sight of, as this will greatly influence their digestibility; thus, lightly boiled albumen is more digestible than hard boiled, &c.; this branch of the subject has, however, already been briefly considered.

As the azotized ingredients of food—i.e., the albuminous—are those which principally contribute to the reproduction of the tissues and organs of the animal system, investigators first directed their attention to the amount of these matters contained in the several aliments Lehmann quotes the tables of Boussingault, Thompson and Schlossberger, and Kemp, &c., in reference to this point, and adds:

"Of the interesting conclusions which may be drawn from these investigations, we will only mention, that the *amount of nitrogen contained in muscular fibre is not essentially different in the entire animal kingdom.* The flesh of fish affords the same absolute amount of nourishing material as that of the higher animals; oysters, on the other hand, contrary to the usual opinion, yield much less; demonstrating the difference which exists between containing an absolute amount of nourishing matter, and containing what is easily digestible.

"We need scarcely mention, that we cannot infer the value of animal food for the reproduction of the blood and tissues from its amount of nitrogen;—for the nitrogen we obtain from it is partly derived from its gelatinous matters, and it is very doubtful whether the latter can contribute anything to the reproduction of the tissues—at least, it follows with certainty, from their composition, that they cannot fulfil the same ends as the proper albuminous substances." (p. 443.)

On the scale constructed by Liebig, showing at a glance the proportion between the albuminous, or plastic, and the unazotized elements of the ordinary articles of the food of man, the former being taken as unity, the author remarks that, as this chemist considers the unazotized matters principally as the generators of animal heat, and as the latter—i.e., the fats and carbo-hydrates—exercise an influence in the production of animal heat, varying in proportion to their amount of oxygen, it would be necessary, in order to simplify the proportion, to make the value of the fat equal to that of the carbo-hydrates by calculating the oxygen; thus, ten parts of fat will about equal twenty-four of starch; sugar of milk and glucose can, of course, be reduced to the corresponding value in starch by calculating the water. Lehmann, however, remarks that, in estimating the relative value of aliments, it would be necessary to distinguish the fat from the carbo-hydrates; for that both are required to form food perfectly

suited to the wants of the animal system is evident, amongst other things, from the universal desire to mix amylaceous articles with fat, and *vice versâ*, and from the presence of both fat and sugar in milk. As yet, we have not ascertained, by decisive experiments, the specific functions of each in the metamorphosis of tissue: Lehmann proposes, until this shall have been done, to take the mean constitution of human milk as the emblem of the combination of those four groups of bodies which is best adapted to the wants of the human organism—i.e., 10 parts of plastic matter, 10 of fat, 20 of sugar, and 0·6 of salts; but in considering this part of the subject, we must not forget that the proportions of the several species of aliment should vary as the wants of the system, under particular circumstances or at different times, may require, of which we have illustrations in the changes which the mammary secretion itself undergoes according to the age of the child, and in the variety presented by the milks of the sundry classes of animals.

Review IX.

Epilepsy and other Affections of the Nervous System, which are marked by Tremor, Convulsion, or Spasm: their Pathology and Treatment. By Charles Bland Radcliffe, M.D., &c., &c.—*London*, 1854. 8vo. pp. 144.

Dr. Radcliffe divides his work into two parts; the first treats of the physiology of muscular contraction; the second of the pathology, periodicity, and treatment of epilepsy, and of the affections which Dr. Radcliffe considers to be allied thereto. The physiology professes to be novel, and to be the basis of the pathology and therapeutics. It is wholly opposed to the current views entertained as to the nature of muscular contraction; we therefore propose to examine it critically, as well as the pathological and therapeutical views by which it is followed; moved thereto, however, rather by a feeling of regard for the author, than by a high estimate of the validity or importance of his doctrines.

Dr. Radcliffe is of opinion that the common doctrine of muscular contraction is erroneous. It is generally believed that it is induced by certain stimuli; such a supposition does not, he thinks, bear the test of examination. The condition of the muscular system known as *rigor mortis*, and the "contraction which occurs in the muscle-like tissue of the ductos under the influence of cold," especially suggested doubts to Dr. Radcliffe as to its accuracy. Dr. Radcliffe's hypothesis is this:

"That all stimulants, vital and physiological, antagonise muscular contraction; and that contraction happens from ordinary molecular attraction, when the muscle is not stimulated."

Or, in other words of the author:

"Because some stimulus has been removed which had prevented the natural molecular attraction of the tissue from coming into play, when the phenomenon becomes analogous to the contraction which takes place in a bar of metal, or in any other inorganic body, on the abstraction of heat."

By this hypothesis the contraction of muscle, the movements of the

blood in vessels independently of the heart, and the action of the heart, receive a (to Dr. Radcliffe) satisfactory "physical explanation."

Dr. Radcliffe builds up an obscure hypothesis on premises still more hypothetical. We will give illustrations of his method of procedure in establishing his doctrines.

"A muscle is said to be irritated, to contract by the simple contact of certain natural or artificial substances—the bowel by the food, a detached fibre by the point of a needle—how is this? What is the influence at work, and what is the manner of the operation? It is difficult to acquiesce in the common belief that the hollow involuntary muscles are excited to contract by their natural contents. If the morsel exerts this influence in the gullet, how can it effect an entrance? Or, if effecting this, how is it that it is not fixed immovably in one place? There is no physical necessity why the movements should follow in any certain and definite order. On the contrary, the morsel may move upward or downward indifferently, as it is seen to do in the throat of a cow during rumination. Indeed, so far from exciting contraction, it appears as if the food remained quietly in the stomach, and in every other part of the alimentary canal, until the digestive process is complete, and that contraction *happens* when those molecular changes are at an end which could have acted as a stimulus to the muscle." (p. 9.)

The entire difficulty encountered by our author arises, we need hardly say, from the circumstance, that he has not rightly comprehended the physiology of deglutition and of peristaltic action. It is very well known that there *is* a necessity, not physical, but *vital*, why the movements of the apparatus called into action during the process should follow in a certain and definite order. It is an essential point, indeed, in all vital processes, that they go on necessarily, according to a pre-ordained and fixed method and plan. Science investigates this order: hence physiologists have traced out the existence of a covering to these hollow involuntary muscles, of incident excitor nerves distributed through and upon that covering, of nerve-trunks taking their course to the central axis where the co-ordinating apparatus is placed, and of other nerve-fibrils coming from that axis and distributed to the muscles which they regulate; all these take their share in the act of deglutition; but the "natural contents" are never in "simple contact" with the hollow and involuntary muscles, which perform by their pre-arranged and combined action the act of deglutition. Nor is it in accordance with the facts elucidated by physiological research to say, that it appears as if the food remained quietly in the stomach until the digestive process is complete, for observations prove exactly the contrary.

We need not multiply extracts by way of illustrating Dr. Radcliffe's method, for his other arguments do not differ in weight or method from the preceding. The following summary of his first chapter ends, however, with a *chemical* argument, as facile and as insequential as his physiological; we therefore subjoin it:

"Contraction, then, as seen in ordinary muscle, would appear to be analogous to that contraction which takes place in inorganic bodies on the abstraction of heat, with this only difference, that more forces have to be abstracted from the organic than from the inorganic body. The analogy is indeed perfect, for even that remarkable degree of contraction which is witnessed in muscle, as compared with that which is seen in inorganic bodies, may be a natural consequence of the physical constitution of muscle: for as muscle is composed almost exclusively of

certain gaseous elements, it *may* contract to a great degree under a small abstraction of heat, because it is the law of its constituent gases so to contract." (p. 24.)

In chapter second, muscular contraction, as manifested in the coats of vessels, is elucidated. *Heat* generated during nutrition and innervation expands the vessels.

"If heat, and the agents associated with heat, excite expansion in the vessels, it is easy to understand how they must give rise to a force—*a 'capillary force'*—which is independent of the heart; and how they must co-operate with the heart in facilitating the progress of the blood through the vessels of the living animal, in precisely the same way as the warm-bath co-operates with the injecting-syringe of the anatomist in procuring the admission of the melted wax into the vessels of the dead animal. It would be different if the vessels acted upon were composed of ordinary solids, for then the heat would cause a greater degree of expansion in the fluids contained within the vessels, than in the vessels themselves; but in reality the vessels are gases, *coerced* into solidity for the time, rather than ordinary solids." (p. 27.)

We never before suspected that the "subject" to be injected was put into warm water for any other purpose than to prevent the melted wax becoming solid too soon, by the abstraction of its heat; and as to the vessels being constituted of *coerced* gases, we would ask, why are they more so than water itself, or albumen, or the *materies* of the blood-corpuscles?

The third chapter treats of "muscular contraction as manifested in the heart;" and here we have a repetition of assumptions as groundless as they well can be. We subjoin an illustration:

"There is reason, also, for supposing that the systole is always contemporaneous with a lessened supply of nervous influence, and not with an increased supply. The blood escapes from the heart in gushes; and there is some reason for believing that the nervous energy, in some parts at least, is generated in corresponding gushes. At all events, the blood passes by gushes into the great cerebro-spinal masses, and causes corresponding pulsations in them whenever they are exposed to sight. It may further be supposed that nervous influence is developed and distributed the moment the blood comes in contact with the organ in which it originates, for this influence is sudden and subtle as electricity. All this may be supposed, and, if so, then the heart will be supplied with the gush of nervous influence, not during the systole, but during the diastole." (p. 32.)

There is no more interesting study than the circulation of the blood through the capillaries; and when the difficulty of the study is considered, there is no branch of physiology in which greater progress has been made. This we owe principally to microscopic research. Never yet, we believe, has the blood been seen flowing through the capillaries by "gushes," nor is there a single phenomenon known which would indicate any such condition of the capillary circulation. We have seen how, in the last paragraph, "all this may be *supposed*." In a page or two, these pure (and unfounded) suppositions assume the shape of "the rational and physical explanation of the heart's rhythm," as if they were demonstrated propositions.

"The heart contracts, and sends a flood of blood to the great sources of innervation. There the blood originates a gush of nervous influence, which, passing along the nerves to the heart, causes the diastole. The diastole cuts off the arterial stream, and the blood sets in from the auricles and veins. The cutting off of the arterial stream interrupts the development of nervous influence, and this interruption, reacting upon the heart, removes the influence which had caused the

diastole, and the systole returns. And so round the same circle. The arterial stream sets out at the systole, and develops the nervous influence at its proper sources, and the nervous influence inducing the diastole causes the systole by cutting off the supply of blood, and by thus suspending for a time the active development of nervous influence. And thus in a regular series systole gives rise to diastole, and diastole is followed by systole, so long as the vascular and nervous systems retain their integrity. This is *one* part of the process." (p. 35.)

It certainly is not. There are no "gushes of nervous influence," and no "cutting off" of the gushes. The whole scheme is purely imaginary.

To determine the PATHOLOGY of epilepsy, Dr. Radcliffe examines "the real state of the great systems chiefly concerned—the vascular, the nervous, and the muscular." In all he finds want of tone. In the vascular system, "all the symptoms are found to offer a direct contrast to those symptoms of plethora which are met with in the butcher." As to the nervous system, all the facts declare the same want of vital activity. As to the muscular, its condition he finds to agree with that of the vascular and nervous systems. Dr. Radcliffe finds—

"Everything is in harmony with the physiological premises; and, as might be anticipated from these premises, the convulsion would seem to depend upon *want* of vital stimulation, which want had allowed the molecular attraction of the muscles to come into play, and gain the ascendency."

Dr. Radcliffe is silent as to the etiology, whether of the predisposition or of the paroxysm.

The affections which are considered as "allied" to epilepsy, are, in fact, neither more nor less than incongruous groups of diseases, in which there is the common symptoms of irregular muscular action, chorea, delirium tremens, poisoning by mercury, hydrocyanic acid, lead, &c.; fever, retention of urea in the blood, tetanus, hydrophobia, &c., are considered as allied affections, in so far as the muscular system is affected. In all, Dr. Radcliffe finds a corroboration of his views.

"In conclusion, therefore, it must be admitted, that the entire history of epilepsy, and of affections allied to epilepsy, is at complete variance with the idea that the muscles are provoked to excessive contraction by excessive stimulation. It is as much at variance with this hypothesis as it is in harmony with that doctrine of muscular contraction which was propounded at the commencement of this inquiry, which doctrine is—that all stimulants, vital and physical, antagonize muscular contraction, and that contraction is brought about by ordinary molecular attraction, when the muscle is *not* stimulated. This doctrine, indeed, supplies the key to the pathology, and the facts belonging to the pathology furnish the only proofs which were wanting of the truth and universality of the doctrine." (p. 108.)

The chapter on periodicity communicates nothing new. Dr. Radcliffe refers the reader for information as to diurnal, monthly, and annual periodicity, either to personal observation, or to "the admirable little treatise of Dr. Mead, 'De Imperio solis ac lunæ in corpora humana et morbis inde animalis.'"*

The chapter on the treatment of epilepsy and the allied affections, needs no special criticism. Dr. Radcliffe recommends a tonic and stimulant plan of treatment; a nutritious and animalized diet; a liberal allowance

* We need hardly state that a little library of books on the subject has been written since the date of Mead's essay; and that it has received a large development through natural history on the one hand, and meteorology on the other.

of wine or beer; coffee, to the exclusion of tea; celibacy; quinine, iron, turpentine, camphor, chloric æther; counter-irritants; hot baths, one every day, &c.

"In actual practice, I have rung changes upon these different stimulants, either giving them alone, or combining them with iron or quinine, substituting one for another, according to the changing circumstances of the case, and always allowing, at the same time, a liberal supply of dietetic stimulants—upon which, indeed, hope is mainly to be based; and I have had every reason to be satisfied with the results. I have never met with a patient who has not been benefited; for even where the case has been of long standing, and the fits have kept their ground, there has been a manifest diminution of intellectual torpor, the face had lost a good deal of the brutalized expression which had been creeping over it, and the distressing nervous headache has disappeared, if that symptom had been present; and I have met with many patients who have been completely cured." (p. 130.)

Dr. Radcliffe has grounds for congratulation as to the success of his treatment, whatever may be thought of his hypotheses. If he had occupied his pages with details of some of the cases he has so happily cured (or if not cured, at least relieved), his work would have had a practical value which it does not possess in its present form. Empirical knowledge of this kind is very estimable in the absence of information as to the true nature of epilepsy—we mean histories of carefully-observed cases; for it is mainly by such histories that the circumstances under which remedies may be prescribed with some reasonable hope of success, can be ascertained. We are sincerely sorry that we cannot express a more favourable opinion of Dr. Radcliffe's little work; for we are assured that he is a gentleman of personal worth and an estimable character. We should, however, do a wrong to our readers, if we were to say more of it than it deserves.

Review X.

Cases of Bright's Disease, with Remarks. By Samuel Wilks, M.D. Lond. ('Guy's Hospital Reports,' Second Series, vol. viii.)

In a previous number* of this journal, we endeavoured to correct what we hold to be the errors of those pathologists who see, in the various abnormal states of kidney which are commonly included under the term "Bright's disease," only successive stages of a single morbid process; the continual tendency of the disease being, as they suppose, to produce that small, contracted, granular kidney, the weight of which is, perhaps, by one-half, or even two-thirds, less than that of the healthy organ. We adduced two classes of facts which appear to us to be quite irreconcilable with the theory of the *oneness* of Bright's disease—these are, *first*, the anatomical characters of the diseased organ, as revealed by a post-mortem examination; and, *secondly*, the condition of the urine, and the attendant symptoms, observable during the lifetime of the patient. If, by an edict of arbitrary power, any pathologist were restricted to the observation of one of these classes of facts, he would labour under a great disadvantage, and he might be expected for a long time to halt between

* Jan. 1853, vol. xi. p. 56.

two opinions with respect to the theory in question; but for those who have the opportunity to examine and compare the clinical history with the pathological anatomy of renal disease, it appears not difficult to arrive at the conclusion that, while the term "Bright's disease" is retained, scientific accuracy and practical utility alike demand a clear distinction between the different and, in a certain sense, antagonistic morbid conditions which are included under it.

The appellation "Bright's disease," as a comprehensive generic term, is one which may very conveniently be perpetuated, and the disuse of which we should regret, if for no other reason, because we delight to honour the name of the distinguished physician who opened to us the great field of renal pathology. But, undoubtedly, the term in question affords a good illustration of that class of idols which Bacon has called *idola fori*.* Is not the idea of unity, of a single disease, implied in the very term "Bright's disease?" This is, unquestionably, the idea which the name conveys to every young pathologist, and the error which it implies can be met and corrected only by teaching him that the term is applied, not to a single disease, but to a *class* of diseases whose existence and whose clinical history were first made known by Dr. Bright.

The intelligent author of the paper which we purpose now to pass in review believing, as he states, that the microscope has led into error some who have trusted too exclusively to its use, "with a view of testing the value of some of the new theories on renal disease," has reviewed his cases; "and, to put them to further proof, he has carefully watched every instance of the disease which has entered Guy's Hospital during the past year." The result of this labour is very creditable to Dr. Wilks as a pathologist and a practical physician With respect to some points of detail, we shall have to express our dissent from his views; but, so far as regards the broad principles of renal pathology, we are happy to find that the erroneous doctrines of Reinhardt and Frerichs, which we endeavoured to refute on a former occasion, find no advocates within the walls of Guy's Hospital.

Dr. Wilks makes it one of the main objects of his communication, to draw a broad line of distinction between the two great classes of Bright's disease—the one being characterized by the "well-known large white kidney," and the other by the "small contracted kidney." With reference to this point, he observes:

"Finding that Frerichs mentions these two conditions as merely stages of the same morbid process, I have been anxious to find his proof, and have failed. Of course, he could not show that any morbid condition of an organ had been preceded by a different morbid condition; but this he might have done—he should have attempted to show that the symptoms, so generally connected with the former, have existed in the cases where the latter had been found. If this were generally, or occasionally, the case, there might have been some ground for believing in the identity of the two; but the opposite is the fact, that, in cases where the small contracted kidney is found, no symptoms of acute dropsy or of an inflammation has existed, such as is found in connexion with the other variety of kidney."

* "But none are so troublesome as the idols of the market, which insinuate themselves into the mind, from the association of words and terms. For though men believe that their reason governs words, it also happens that words retort and reflect their force upon the understanding" (Novum Organum.)

The case thus broadly stated is undoubtedly true in so large a number of instances, as should suffice to convince an unprejudiced inquirer, even without the help of other evidence. We think, however, that in this, and in some other passages of his paper, Dr. Wilks has made the distinction between the clinical history of the two classes of cases more absolute than it actually is. The large white kidney is not unfrequently a chronic disease, commencing insidiously, without exposure to cold or the previous occurrence of inflammatory symptoms, and sometimes even making considerable progress before the occurrence of dropsy excites a suspicion of renal disease. Then, on the other hand, we have seen more than one case of contracted kidney in which we had good evidence that the commencement was an attack of acute dropsy, occurring in a person previously healthy. Again, it must be borne in mind that, before the termination of many cases in which the small contracted kidney is found after death, the dropsy is as great and as general as in any case of the large white kidney. The chief distinction between the two classes of cases is this—that whereas, according to our experience, the large white kidney *never* proves fatal without the previous occurrence of dropsy—which is, in fact, usually one of the most prominent and distressing symptoms—the small contracted kidney, in many instances, proceeds to its extreme limit of degeneration, and at length destroys its victim without giving rise to dropsy in any form or in any degree: a fact which we hold to be totally irreconcilable with the hypothesis which assumes that the small contracted kidney is only a more advanced stage of the large white one.

An examination of the urine, as to quantity and quality, in the two classes of cases, affords the means of explaining the much more frequent occurrence of dropsy in connexion with the large white kidney than in cases of the small contracted kidney. In the former class of cases, the urine is almost invariably less copious than in health, and it contains a large amount of albumen; whereas, in cases of the small contracted kidney, the quantity of urine, as a rule, is considerably above the normal standard, while its albuminous contents are much less than in the other class of cases; so that the quantity of albumen which is drained off from the blood is widely different in the two forms of disease, and this not only relatively to the quantity of urine, but absolutely as to the amount of albumen discharged in the twenty-four hours. Now, clinical observation is continually teaching us that the risk of the occurrence of dropsy is, *cæteris paribus*, in inverse proportion to the quantity of urine secreted, and in direct proportion to the impoverishment of the blood, which is occasioned by the escape of its albumen through the kidneys. In accordance with this general statement is the fact that, in the advanced stages of the small contracted kidney, a sudden diminution of the quantity of urine is a frequent precursor of dropsy, and of a speedily fatal termination.

With reference to the character of the urine in the two classes of cases, one point of distinction is to be found in the *density* of that fluid. The urine secreted by the large white kidney has a density rarely below 1·015, and it ranges from that point to 1·025, or even as high as 1·030; while in cases of the small contracted kidney the density is more frequently below than above 1·015, varying from this point to 1·010, or even as low as 1·005. The high density in the former class of cases results from a scanty secre-

tion of liquid, with an abundance of albumen, and a more or less efficient excretion of the normal constituents of the urine; while the comparatively low density in the other cases is chiefly due to the excess of liquid. It is obvious that the actual amount of urinary solids eliminated within a given time may be greater with a copious secretion of urine of low specific gravity than when the density is high, but the measure in a more than corresponding degree scanty.

Proceeding further with this investigation, we shall find in the pathological anatomy of the kidney some explanation of those remarkable differences in the quantity and quality of the urine to which we have referred as being characteristic of the two great classes of Bright's disease.

One of the most remarkable points of difference between the two forms of disease, is to be found in the condition of the epithelial lining of the uriniferous tubes. In that form of disease which leads to the small contracted kidney, the gland-cells become disintegrated, detached from the basement membrane, and finally washed out with the urine. The tubes are thus either entirely denuded, or they are found to be lined by a layer of delicate cells, entirely different from the normal epithelium. It appears in the highest degree probable that a tube in either of these conditions, while it has lost its power of secreting the solids of the urine, may yet retain that of separating the watery constituents from the blood; and that in this pathological fact is to be found the explanation of the abundant flow of urine, pale in colour and low in density. Thus much is, at any rate, certain, that with respect to the condition of the tubes and their epithelial lining, the small contracted kidney stands in the most marked contrast to the large white kidney. In the last-mentioned form of disease, the gland-cells are never so detached, disintegrated, and swept away as to leave the tubes denuded. They remain adherent to the basement membrane, and undergo changes more or less considerable in different cases, varying from a slight granular opacity to a complete oily degeneration, or they become replaced by an albuminous or fibrinous material, which more or less fills the tube. In this form of disease, then, that condition of the tubes which we have referred to as being apparently so favourable for the transudation of water, is entirely wanting.

The large amount of albumen secreted by the large white kidney, and the comparatively small quantity separated by the contracted kidney, is probably in great measure dependent on the relative degrees of vascularity of the gland in the two forms of disease. In the large kidney there is often an excess—an actual hypertrophy—of glandular tissue; and the number of pervious bloodvessels, if not greater than in the healthy kidney, is rarely in any considerable degree less than normal. In the contracted kidney the opposite condition is found; for as the disease advances, many of the uriniferous tubes shrink, and the vessels which supply them —both arteries and Malpighian capillaries—may be seen to have their canals obstructed, and their walls covered with oil globules. To such an extent does this proceed, that in the later stages of the disease the kidney is reduced to the condition of an organ but scantily supplied with blood; and, obviously, in the same proportion the materials for a copious secretion of albumen are wanting.

More than once in the course of his paper, Dr. Wilks alludes to the

fact of the urine being entirely free from albumen in cases of renal disease, and he states that he has frequently found it so "in the chronic degeneration;" that is, as we understand him, in cases of the small contracted kidney. This statement, unqualified and without explanation, appears to us calculated to mislead. It has frequently been asserted by authors of deservedly high repute, that the occasional absence of albuminuria is a fact of not uncommon occurrence in the *advanced stages* of this form of disease. Our own experience leads us to doubt the accuracy of this statement, and to class it with those traditional errors—by no means uncommon in the science of medicine—which have passed current from one writer to another without having been subjected to a sufficiently rigid scrutiny. Although in the later stages of this form of disease the absence of albumen is an occurrence which we do not remember to have observed in a single instance, yet in the earliest stages a microscopic examination of the urine not unfrequently affords unequivocal evidence that the morbid change in the kidney has commenced and is in progress, while as yet the most careful examination can detect no trace of albumen in the urine. With reference to this point, then, we believe the truth to be that the urine may contain no albumen until the renal degeneration has made considerable progress; that in the more advanced middle stages the quantity of albumen is in proportion to the activity and rapidity of the disease; and that, again, in the latest stage of the renal degeneration the albumen is present in less quantity, for the reasons before stated. If during this period of the disease the urine be at any time entirely free from albumen, this is unquestionably an event of rare occurrence.

And here it may not be out of place to observe that, for the detection of minute quantities of albumen, considerable attention and care are necessary to avoid some sources of fallacy. It should be borne in mind, that heat is decidedly a more delicate test than nitric acid, when the urine is very slightly albuminous. A quantity of albumen, so small as to be readily decomposed by nitric acid, will be easily detected in *acid* urine by the careful application of heat. After coagulation by heat, a few drops of acetic acid should be added, to avoid the error of mistaking a sediment of earthy phosphate for coagulated albumen. If a drop of nitric acid be added for the same purpose, this is best done after the urine has been allowed to cool, lest, at a high temperature, a scanty coagulum of albumen be decomposed by that acid. Within the last few days, we nearly failed to detect the presence of albumen under the following circumstances. The urine was acid, and when boiled, it deposited rather copiously a white flaky precipitate; on the addition of nitric or acetic acid, this was rapidly dissolved, and we concluded that the sediment had been saline, and not at all albuminous. On microscopical examination, the urine was found to contain a considerable number of tube-casts, and this discovery led to a repetition of the tests for albumen, when it was ascertained, that although the sediment caused by boiling was in great part phosphetic and dissolved by acids, there yet remained a scanty, though unequivocal, albuminous coagulum, which was insoluble both in acetic and nitric acid. The urine is occasionally rendered turbid by the admixture of leucorrhœal or other discharges. The detection of a scanty albuminous coagulum in such a specimen will be much facilitated either by filtering

the urine before the application of tests, or by allowing it to remain at rest until the deposit of extraneous matters has cleared the supernatant liquid, which may then be submitted to the tests.

We have hitherto spoken of two forms of Bright's disease, and we have indicated some of the principal features by which they are characterized and distinguished. With regard to the question of a more minute classification, our author expresses himself dissatisfied with all attempts which have hitherto been made in that direction. He, however, is not prepared with any subdivision or nomenclature which he can recommend as being more worthy of general adoption than those which have been proposed by others. The term "coarse," which he uses to express certain external appearances of the kidney, is too vague and ill-defined to have any value beyond the post mortem room of Guy's Hospital; and the "fibrous degeneration" of which he speaks as a fourth form of disease, is, to say the least, so rare an occurrence, that it may conveniently be excluded from consideration, until we can agree as to the nature and the name of some morbid conditions of far greater frequency and importance.

Now, it is an essential element of success in any undertaking, that we should have a clear and definite notion of the object which we desire to accomplish; and just in proportion to the doubt and confusion which may exist as to this point, will be the risk of the failure, which results from our efforts being misdirected. With reference, then, to the subject which we have now under consideration, it is important to determine for ourselves what is the object which we seek to attain by a subdivision of the forms of Bright's disease, beyond that into the "large white kidney," and the "small contracted kidney;" this primary distinction being one respecting which we agree with our author, in thinking there can scarcely be a reasonable doubt.

In the first place, it should be clearly understood, that the term *specific*, in the sense in which that term is defined by zoologists and botanists, is not applicable to any of the distinctions between the different forms of Bright's disease. There are some diseases which, with strict propriety, are called specific; inasmuch as not only are they distinguishable from other diseases by well-defined and easily-recognised characters, but they have also the power of perpetuating the species, by the reproduction of disease, in all essential respects identical, in the bodies of other living beings. As illustrations of this class of diseases, we need only mention measles, scarlatina, and small-pox, amongst acute diseases, and syphilis amongst chronic maladies. Now, in this sense of the term specific, it might be a question how far it is strictly applicable to many forms of general and local disease, the distinction and the definite nomenclature of which is yet a matter of much practical importance. With respect, however, to the forms of Bright's disease, there can be no question that the term specific is inapplicable, and the attempt to apply it in this case would be only to introduce a new element of confusion, without the prospect of any compensating advantage.

It being, then, admitted that the forms of Bright's disease are not specifically distinct from each other, it remains for us to determine in what way, and for what purpose, their subdivision is to be effected.

Suppose now the following statement to be a correct expression of

facts—that there are certain morbid conditions of kidney, readily distinguishable from each other by a post-mortem examination—that by a microscopical and chemical examination of the urine these several morbid conditions of kidney may be recognised and distinguished, during the lifetime of the patient, with scarcely less ease and certainty than when the diseased organ itself is exposed to view,—further, that certain of these morbid states are of far more serious import than others, either as being originally and essentially more formidable in character, or as indicating a more inveterate and advanced degeneration of tissue.

If it should be found, upon careful inquiry, that these propositions can be admitted as established facts, then, whatever may be said by the mere morbid anatomist, who breathes only the atmosphere of the dead-house, we are confident that the pathologist and the physician will at once recognise the importance of distinctions which may afford valuable aid in the diagnosis and prognosis—nay, even in the treatment—of renal disease. It is not our intention now to enter at length upon the consideration of the various forms of Bright's disease—we have fully discussed that subject elsewhere—but in adverting to some points of difference between Dr. Wilks and ourselves, we shall briefly state certain facts which must, as we believe, form the basis for the subdivision and the nomenclature of the various forms of the disease in question.

Dr. Wilks, in commenting upon some terms which have been proposed by the author of this review says:—"the terms [*acute and chronic desquamative nephritis*] convey some meaning, and some real facts; but if the former apply to an acute inflammation of the tubes of the kidney, and the latter to a chronic inflammation, these expressions would be better." Now, that the term acute inflammation is not sufficiently precise will be evident from a consideration of the following facts. Two cases of dropsy after scarlatina present themselves to our observation. In their general features both cases are alike; in both the urine is scanty and highly albuminous; but on a microscopical examination, we find in one case that the urine contains, besides blood-corpuscles and tube-casts, numerous cells of renal epithelium, partly entangled in the casts and partly scattered through the urine; in the other case we find that the tube-casts entangle, not epithelial cells, but pus-corpuscles, many of which are also scattered over the field of the microscope. Now, this remarkable difference in the microscopical characters of the urine in the two cases is not accidental but essential; and if we watch these urines from day to day, we shall find that they retain their distinctive characters: only, during the progress of the disease, undergoing certain modifications which we need not now particularly describe. There probably are few pathologists who would hesitate to give a distinctive appellation to two forms of disease thus characterized; and the physician finds an additional motive for distinguishing them, in the fact that one form of disease is much less curable, and much more frequently fatal, than the other. Now, in both sets of cases there is what may be called "acute inflammation of the tubes of the kidney;" and in order to express this fact, and also to distinguish one form of inflammation from the other, we have proposed to call one "acute *desquamative* nephritis," the other "acute *suppurative* nephritis." In short, to the expression which Dr. Wilks considers alone sufficient,

we prefix another, for the sake of marking a real and a very important distinction.

In a third case of dropsy after scarlatina, the urine may present appearances different from those in either of the cases to which we have just now referred. The urine is scanty, and highly albuminous; it deposits little or no sediment, and on microscopical examination it is found to contain neither pus nor renal epithelium, there being perhaps only a slight cloudy deposit, composed of transparent fibrinous casts, the small diameter of which is evidence that they have come from tubes which retain their epithelial lining, the casts having been moulded within that space in the tubes which is immediately bounded by the gland-cells. Now, again, this character of the urine is not accidental, nor does it indicate an earlier stage of either of the two conditions which we have before described; it usually remains the same from day to day, and if the case terminates fatally, as these cases not unfrequently do, there is found, as might be expected, an intimate relation between the condition of the kidney and the characters of the urine. The kidney-tubes are not filled with desquamated epithelium, as in the first case, nor transformed into pus, as in the second, but they retain their position in the tubes, only being more opaque, granular, and bulky, than in the normal state; and this slightly altered condition of the epithelium, with some opacity of the Malpighian capillaries, and here and there, perhaps, a tube containing extravasated blood, constitutes the entire morbid anatomy of this form of disease, which it has been proposed to call *non-desquamative.* We are not prepared to maintain that this term is better than any other which might be proposed, but it is certainly expressive of a fact which is of essential importance in the pathology of the kidney—namely, the absence of epithelial desquamation. It appears to us that if a name can be made to express the most important feature of the morbid condition to which it is applied, this is as much as can be expected; since it is not possible to condense into one or two words the entire history of a disease.

Our limits will not permit us now to enter upon the question of the proximate cause and the pathology of the desquamative process in the kidney; but this point we beg to submit for the consideration of our readers, with the hope that they will be induced to test the accuracy of our statement—that the acute desquamative disease is in general a much more favourable form of disorder than either the non-desquamative* or the suppurative. Happily, too, it is the most frequent, not only in cases of dropsy after scarlatina, but in all cases of acute renal dropsy, from whatsoever cause arising.

Now, if we do not mistake the language of Dr. Wilks, he objects to any system of nomenclature for renal disease which dissevers "scarlet-fever dropsy cases." Such an objection, it appears to us, can only be based upon a misapprehension of the object for which cases of Bright's disease may usefully be subdivided. If our subdivisions and our nomenclature are to have reference, not to the pathological conditions of the kidney

* The comparison between the desquamative and the non-desquamative process can be fairly instituted only with cases in which the urine is equally and copiously albuminous. There are some cases of slight renal congestion in which, with a small quantity of albumen in the urine, there is neither desquamation nor sediment of any kind; these cases are, obviously, not comparable with the more severe forms of disease.

itself, but to the nature of the constitutional disorders which give rise to the renal disease, the result will obviously be, that under one name—of scarlet-fever kidney, for instance—will be included very different morbid conditions; while to pathological states of the kidney, which are essentially alike, would be applied different names, inasmuch as the same form of disease in the kidney—for instance, acute desquamative disease—may result from scarlet fever, from measles, from erysipelas, from exposure to wet and cold, and from various other causes. And, be it observed, we do not find a sufficient reason for withholding from the various morbid states of kidney to which we have referred, or shall hereafter refer, a distinctive appellation, in the fact, that one condition may sometimes be seen to pass into the other, or that two morbid conditions may coexist in the same kidney. If this were a sufficient reason, then it would apply with at least equal force in the case of diseases of the liver or lung, most of which might, on such grounds, be included, without further distinction, under the comprehensive terms, hepatic or pulmonary disease.

With respect to the chronic varieties of Bright's disease, we have proposed to call that form of disease which results in the small contracted kidney, *chronic desquamative disease*—not, as Dr. Wilks repeatedly states, *non*-desquamative disease.* The name is expressive of a process which is a constant feature of the disease—namely, a continual scaling off and crumbling away of the renal epithelium, which the urine, after standing for a short time, deposits as a white sediment. On microscopical examination, the disintegrated epithelial fragments may be seen partly scattered, and partly in the form of granular tube-casts. These tube-casts are of great diagnostic value, for they are often present, and indicate the existence of commencing renal degeneration, before the occurrence of albuminuria; and, at a later stage, their number affords a tolerably accurate index of the rate at which the disease is making progress. We gather from Dr. Wilks's general remarks, as well as from the brief reports of his cases, that he finds no microscopic sediment in the urine in connexion with the small contracted kidney. This is a point which we have examined with much care in a large number of instances, and it will be seen that the result of our experience, being such as we have just now stated, is in direct opposition to that of Dr. Wilks.

The cases of chronic disease in which there is either no sediment or only a light cloud, which subsides after the urine has remained for a few hours at rest, are those to which the term *chronic non-desquamative disease* are strictly applicable. The urine is of comparatively high density, and contains an abundance of albumen, and the kidneys are large and pale. The cloudy sediment in the urine contains small wax-like fibrinous casts. In some cases, the kidneys undergo a further change; the epithelium, in certain sets of tubes, suffers a fatty transformation; this change is indicated in the urine during life by the appearance of oil in the cells and casts, and in the kidneys, after death, by the peculiar yellow granulations, like small specks of atheroma, which are scattered through the cortical substance. If any term more expressive than that of *granular*

* Thus, at p. 303, Dr. Wilks says, "Surely an organ which has undergone such a change or degeneration as we witness in the small granular kidney, deserves a more significant name than the negative one of non-desquamative." Here the negative is introduced by Dr. Wilks, and his criticism is so far misapplied.

fat kidney can be suggested for this form of disease, we shall very gladly adopt it in place of that by which we have proposed to distinguish this from other forms of "the large white kidney."

To recur once more to the end and object of these subdivisions: Why—it may be asked—do you make a distinction between the chronic non-desquamative disease, as you are pleased to call it, and the granular fat kidney, since you admit that the latter is, sometimes at least, only a more advanced stage of the former? Our reply is, that the distinction affords an important aid to the physician in the formation of his prognosis, and therefore, sometimes, in the choice of remedies. Two cases of dropsy, in all their outward appearances alike, may come under observation. In both the urine is of nearly the natural colour, but highly albuminous; both specimens deposit a light cloud after standing, and this cloud, in both instances, contains small, transparent, waxy casts. The important distinction is this—that while, in one case, the fibrinous casts contain only here and there a fragment of epithelium; in the other case, a large proportion of the casts entangle oil, partly in the form of scattered globules, and partly contained in modified epithelial cells. In the first case, a cure must be perseveringly attempted, and may, in fact, be reasonably expected; in the second instance, recovery is scarcely to be hoped for. From the first condition, we know of numerous and complete recoveries; from the second, hitherto, not one. We would remind our readers that often, during the convalescence from acute desquamative disease, a certain amount of oil appears in the urine, but the history and the associated appearances in the urine will serve to distinguish these cases from those of confirmed fatty degeneration of the kidney.

There are few subjects of inquiry more interesting than the question, as to the possibility of tissues which have undergone fatty degeneration being restored to their normal condition. It appears that, in a large proportion of fatal cases of delirium tremens, the muscular substance of the heart has undergone more or less of fatty transformation. The question then arises, may not the hearts of some of those who recover from that disease have been for a time in the same condition of fatty degeneration? Or may it be that, upon the presence or the absence of this structural change in the heart—obviously a very serious complication—depends the curability of delirium tremens? It is evident that the difficulty in the way of giving a definite answer to these interesting queries arises from the fact, that we have no means by which to determine with certainty, during the patient's lifetime, either the presence or the absence of fatty degeneration of the heart. Now this difficulty does not exist with regard to that form of fatty degeneration of the kidney to which we have alluded, under the name of *granular fat kidney*. And those who are desirous to observe the commencement and the progress of fatty degeneration, with a view to determine the extent to which it may proceed before it becomes irremediable, can find no field of observation so favourable for the study as the urine of patients who are threatened with this form of disease. We do not imagine that fatty degeneration of muscle is identical with the similar change in glandular tissue, but there must be an analogy between the two; and every well ascertained fact with regard to one may serve to throw light upon the other.

Returning now to our author, it may be observed that his account of the general history, the complications, and the treatment of Bright's disease, although containing little that is new, yet affords satisfactory evidence that he is making good use of the wide field of observation which is open to him at Guy's Hospital. We have marked a few passages for comment.

With respect to the prognosis in cases of the large white kidney, Dr. Wilks remarks,

"That if, after an attack of acute dropsy, recovery do not shortly take place, but after a lapse of time renal disease still exists, severe symptoms must again be speedily expected, and that three years will be the maximum of time in which the patient has to live."

We do not doubt that in the circumstances referred to many cases will prove fatal within a period of three years, but that a considerable number will live much beyond that period we can testify from our own experience. In the course of last year we attended a patient who died with Bright's disease, which originated in an attack of dropsy after scarlatina ten years before. The urine was albuminous during the whole period. Two years since we examined the urine of a medical man who had dropsy after scarlatina, sixteen years before. He recovered from the dropsy, and thought no more of his illness, until his urine was accidentally discovered to be albuminous five years afterwards by a fellow student. From that time to the period of his visit to us, his urine had not ceased to be albuminous, and it is probable, as he himself believes, that albuminuria had existed since the attack of dropsy. These cases will suffice to show that Dr. Wilks must extend his maximum period considerably beyond three years.

Our experience is quite in accordance with that of Dr. Wilks, to the effect that a complaint of *pain in the loins* is the exception in cases of Bright's disease. In some few acute cases, pain in the region of the kidney is severe, but in by far the greater number of chronic cases the patients not only assure us that they have no pain, but frequently have they urged this fact upon our attention, with the expression of a doubt whether an organ so entirely free from pain can be the seat of any serious morbid change.

With regard to the nature and origin of renal cysts, which often occur in connexion with the chronic desquamative disease, Dr. Wilks appears to be in the unhappy position of a man who has forsaken one creed before he has firmly grasped another. He has lost confidence in Mr. Simon's theory, that the cysts in question are modified epithelial germs; but his assent to the contrary statement, that they are merely altered portions of uriniferous tubes, is interfered with by the fact that many of the cyst-like appearances are much smaller than the normal size of the tubes. This apparent difficulty will be removed if he will take into consideration the fact that a wasted and contracted condition of the tubes is a more frequent result of the destruction of their epithelial lining, than the opposite state of dilatation. Dr. Wilks says:

"It has been taken for granted that the larger cysts found so often on the surface of the kidney are of the same formation as the microscopic ones. Of this there can be no positive proof."

We, on the contrary, are of opinion that the point in question is sufficiently proved by the fact, that the tubes may be seen dilated in every degree, from the normal size until they become visible by the unaided eye. There can be no other proof than this, but surely this is sufficient for the purpose.

Dr. Wilks lays much stress upon the existence of a rigid and tortuous condition of the arteries throughout the body as a concomitant of Bright's disease, and with reference to this subject we find the following passage:

"Dr. Johnson speaks of the renal artery being especially affected in Bright's disease, and caused by its increased efforts in the propulsion of blood through the Malpighian tufts This I cannot admit from my own experience; I have very frequently found it diseased, but never without the same evidence of disease in other arteries. I am obliged, therefore, to put the disease of the renal vessel down to a general cause, and not arising necessarily from any proximity to the organ which is supposed to produce the mischief"

We gather from this passage, that Dr. Wilks has misapprehended the observations of the author whom he quotes. The observations in question relate, not to the trunk of the renal artery, as Dr. Wilks appears to suppose, but to the minute microscopic branches, the muscular coats of which, in the advanced stages of all forms of Bright's disease, are hypertrophied in a very remarkable manner. The change in question has nothing corresponding with it in the coats of the larger vessels. And whatever may be the correct interpretation of this peculiar condition of the minute renal arteries, the anatomical fact itself is so easily demonstrated by the aid of the microscope, that if it has not been seen, this can only be because it has not been carefully looked for.

Our author's remarks on treatment are judicious. He appears, however, to have a remarkable unwillingness to confine his patients to bed and within doors. He cites the case of a man with acute dropsy who, contrary to orders, went out—the weather, however, being warm—and rapidly recovered. "Very frequently," Dr. Wilks says, "I have seen patients with Bright's disease put to bed, and there have died, who I believe would have continued alive for a long time if they had had a moderate share of exercise." It is sometimes difficult to determine whether a patient at a particular period of his illness should be allowed to go out, but it is safer to err on the side of caution; and for every instance of a patient being benefited by disobeying our injunctions to remain at home, we could refer to at least three others who have thereby incurred a serious and even a fatal relapse. Dr. Wilks doubtless intended his observations to apply chiefly to cases of chronic disease, in the treatment of which exercise in the open air is often of service; but it appears to us that they are less guarded than is desirable, in reference to a point of practice about which both patients and practitioners are often more negligent than is consistent with safety.

And now, having referred to the chief points in Dr. Wilks's communication, and having criticised it, as we trust, in no unfriendly spirit, we may remark, in conclusion, that any one who determines to read the paper itself, may do so with the assurance that his time will not have been unprofitably bestowed upon it.

George Johnson.

Review XI.

Sudden Death. By A. B. Granville, M.D., F.R.S., M.R.C.P. Lond., &c., &c.—*London*, 1854. 8vo. pp. 286.

Although this work is intended as much for the public as for the profession, its contents are in many respects such as to merit the attentive regard of the latter. Moreover, where it addresses itself to the former, our jealousy of popular semi-medical writings does not find grounds for objection so much in the topics as in the manner in which they are handled. The subject being such as does most seriously touch the dearest interests of all persons, medical or non-medical, the proverb, *homo sum nihil humanum alienum a me puto*, may be allowed to lay its weight in the popular scale, when we carefully balance the wisdom or the folly of addressing the non-professional public upon professional matters.

In laying the contents of this essay before our readers, we shall follow the author's division of his subject, which he introduces by a few preliminary observations upon the necessity that he conceives to exist for bestowing greater attention upon "head-diseases," which he opines have not heretofore received sufficient attention (!?). However, the deficiency is to be supplied at some future time by Dr. Granville. Let us, then, wait patiently for the fruits of Dr. Granville's experience, derived from "a career of thirty-five years in the metropolis." In the author's own words, the promised and the present work are to be received "as a respectful tribute of accumulated professional experience due by me to the public, from whom I have received every encouragement during a career, &c." We should have more cordially hailed the expression of such a tribute of gratitude, had it been made to the members of a *profession* who are competent to judge of medical facts and reasoning, rather than a public, who can equally admire homœopathy, mesmerism, table-turning, and spirit-rapping.

Pass we now to the book itself, in its own order of subjects. After some introductory remarks, which might have been omitted without detriment, Dr. Granville gives us a description of the arrangements of the office of the Registrar-General. From the records there to be found, Dr. Granville extracts several of the more striking incidents accompanying the returns of the district registrars, *more* Dickens.

Death statistics contain, first, the general mortality of all England, during the ten years elapsed between the two last censuses, with the corresponding mean population of each year, corrected by calculation. Secondly, the number of deaths, under the three heads of sudden, apoplexy, and paralysis, which have occurred in all England in the course of a period of three years. Lastly, the total number of the same kind of deaths that have taken place in the metropolitan division during the same periods, distinguishing the sexes. In this section, instances of sudden death are also quoted from the returns.

From the statistics of the Registrar-General, it appears that the total deaths in all England and Wales, in ten years, have been 4,220,723. The increase of population over deaths during the same period, 2,013,461. From the same records, Dr. Granville shows that a high mortality of one

year is counterbalanced by a low average of another, as it were, striking the average. Thus the mortality of London, in 1849, was 68,432; in 1850, it was 48,579; this mortality being inferior to that of each of the three years preceding cholera. The average is seen in the following statistics for the metropolis:

1847 and 1848—116,759 = average, 58,378.
1849 and 1850—117,011 = average, 58,505.

This average is found equally in a similar calculation for the whole of England. The fact is one of great interest, and Dr. Granville has our thanks for having brought it under our notice. It is to be regretted that the author should have connected these facts with such inferences as are exhibited in the following remarks, characterized, in our opinion, by bad taste.

"It were well that those philanthropists, who are running the *whole hog*, to use a vernacular expression,* with their theory of cholera being the offspring of filth and insalubrious localities, requiring large measures of Government interference, expensive Boards of Health, and extramural interments, should reflect on these statistical facts, and the conclusions to which they lead."

Dr. Granville, not a little presumptuously, pretends to know that the ways of God are as our ways, or else it must be that he consoles himself with a sort of pseudo-Christian fatalism, under a dispensation of which himself holds the authentication. Thus we are authoritatively informed by him, that plagues are not sent "to depopulate whole countries, or to destroy mankind; for when the great plague has passed away, and on the following years 'the tribes are again numbered,' fewer than usual of the people are found to have died in that year, that the 'mass of living flesh' may continue the same. Such is the Divine Covenant." We shall not follow the author in his criticisms upon the reports of the Board of Health—which hence are obviously not likely to find favour in his eyes. We have accompanied Dr. Granville already too far in a digression rather wide of his subject. Returning, then, to the matter in hand, we learn that the number of sudden deaths, as well as those from apoplexy and paralysis, for England and Wales, during the five years 1847-51, has been 92,774; out of which, 15,054 occurred in the metropolis. These numbers, if examined for a series of years, show a progressive increase in the deaths from these causes, over and above what might be expected from increase of population. These deaths are also more frequent in the winter and autumn quarters than in the spring and summer seasons. These statistical results resemble, Dr. Granville points out, those given in a former number of the 'British and Foreign Quarterly Medical Review,' as observed in Italy. It appears that the middle ages—i.e., from twenty-five to sixty, afford the greatest number of deaths from apoplexy. As regards sex, Dr. Granville is of opinion that sudden death from apoplexy is as frequent in females as in males, while paralysis is more frequently the cause of death in the former. The general result of Dr. Granville's investigations under the two last heads is, that from infancy to manhood, or mature age, the deaths from these causes are fewest in number; that

* The italics are ours. We would remind the author that a wide difference exists between a slang, and a vernacular expression.

Still-births are omitted from the registration returns. The number of deaths occurring at each of the first three months are not specially noted, but included in one general enumeration. Dr. Granville follows the number of early deaths through the variations attending locality, status of inhabitants, &c.; and a painful impression is left upon the mind by the perusal of the details adduced, as to the fearful demoralization and indifference to human life manifestly prevailing among the inhabitants of some districts in Great Britain—not in China, but in the British metropolis. The infantile mortality of Bethnal Green is to that of St. George's, Hanover-square, as $6\frac{1}{8}$ to 1 !! Is comment needed? Still more solemn is the lesson inculcated by the state of things in Preston, up to a very recent date, as set forth by Dr. Granville.

In this section, the author has taken the opportunity of offering suggestions with regard to the registration of deaths, more particularly of those under one year, which, if acted upon, would tend to the enhancement of the value of the already highly-valued documents of the Registrar-General's office.

"*Frequency of sudden death.*"—In order to impress his readers with the truth of his statements, and to fix their attention upon the subject, Dr. Granville has brought together a series of "*Facts.*" These facts however consist of what appear to us to be newspaper paragraphs and obituaries, recording sudden deaths; and which, by dint of scissors and paste, might have been almost indefinitely added to, including other equally impressive and striking occurrences.

"*What is sudden death?*"—To give the answer to his question imposes upon the author the necessity of discussing the preliminary question, "What is life?" In so doing, Dr. Granville criticises the principal doctrines of vitality, the definition adopted by himself being that "Life is the communication of an immaterial principle to the organized being, forming no part of it, but simply using it as a machine subordinate to its will."

The answer to the first question, "What is sudden death?" is promised to be more fully given in a future work, which shall treat practically of the causes, and their treatment, and the prevention of sudden death. For the present, Dr. Granville is content with the statement that there is no such thing as *sudden* death, except as the closing scene of a series of pathological changes going on for a longer or shorter period of time.

The conclusion of the present volume consists of a summary of what is contained therein, for the purpose of putting on their guard those individuals who are "likely to be the designated next victims, by showing, 1st, That in our days apoplectic seizures, &c., are more common than in former times. 2ndly, That neither youth nor manhood goes free, but rather the contrary, from such calamities. 3rdly, That these are not confined to the larger masses, but reach the home of the better and most exalted. Lastly, That circumstances attending the sudden deaths of infants are brought to light which demand further investigation."

Review XII.

1. *Bericht über die elektro-physiologische Arbeiten des* Dr. Duchenne, *de Boulogne, zu Paris.* Von Dr. Hermann Eberhard Richter. ('Schmidt's Jahrbuch,' No. 11, 1853.)

Report upon the Electro-physiological Researches of Dr. Duchenne. By Dr. H. E. Richter. ('Schmidt's Jahrbuch' for October, 1853.)

2. *Die Electricität in ihrer Anwendung auf practische Medicin.* Von Dr. Moritz Meyer, arzt in Berlin. 1854.

Electricity in its Application to Practical Medicine. By Dr. Moritz Meyer, of Berlin. 1854.

3. *Histoire de l'Electricité Médicale.* Par M. J. Guitard, M.D., Président de la Société Médicale d'Emulation de Toulouse. 1854.

History of Medical Electricity. By M. J. Guitard, M.D., Toulouse. 1854.

4. *De l'Action Thérapeutique de l'Electrisation Localisée dans le Traitement des Paralysies Consécutives à l'Hémorrhagie Cérébrale.* Par M. le Dr. Duchenne, de Boulogne. ('Bulletin Général de Thérapeutique Médicale et Chirurgicale,' Mars et Avril, 1854.)

On the Therapeutic Action of Topical Electrisation in the Treatment of Paralysis, consecutive to Cerebral Hæmorrhage. By Dr. Duchenne.

5. *Heilung der Impotentia virilis, mittels Electricität.* Von Dr. B. Schulz, in Wien. ('Wien Wochenschrift,' 10—12. 1854.)

On the Cure of Impotence by means of Electricity. By Dr. B. Schulz, of Vienna. ('Schmidt's Jahrbuch,' August, 1854.)

Although Scribonius Largus attempted the cure of gout and headache by placing the feet of his patients in a bucket of torpedos, and Paracelsus recognised some therapeutic properties in the magnetic bar, it was not until the middle of the last century that electricity was employed with any zeal for the cure of disease. The discovery of the Leyden phial at this time gave fresh impetus to the application; by its aid success was obtained beyond the most sanguine hopes; anticipations were raised; great pretensions made, and followed by still greater failures; until what was wanting in fact had to be supplied from fiction; and electricity, in its therapeutic relations, passed from the earnest inquirer after truth, through the hands of the enthusiast, into those of the most heartless charlatans.

Falling into the same category of disgrace with the talisman, amulets and sympathetic cures, its scientific history was for some time lost; and nothing was heard of it, beyond vague rumours of miraculous cures from the shady regions lying outside the circle of sincere investigation and legitimate therapeutics.

In an earlier number of this review,* attention was directed to the researches of Mr. Donovan, Dr. Golding Bird, and others, who had submitted the claims of electricity to renewed examination. It was then shown that many cases of chorea, paralysis, and of amenorrhœa were amenable to this form of treatment, when others had proved unsuccessful.

* Vol iii. p. 373. 1849.

Since that time, scarcely anything of importance has been added by the physicians of our own country; but in France and Germany there has been great attention bestowed upon the subject; and if no new facts have been added, the real use of electricity has been established, the several methods for its application have been tested, and their relative value pointed out. M. Duchenne (of Boulogne) has been the most vigorous labourer *in this field, and it is our special object at the present time* to present the results of his inquiries. Before doing so, we have to speak highly of the history furnished by M. Guitard, to which we are indebted for some of the following information.

After the discovery of the Leyden phial, and before the time of Volta, i. e., from the year 1745 until the commencement of the present century, friction-electricity was the only form used in medicine. Sparks drawn from the "prime-conductor," or from the individual, placed upon an insulated stool; shocks from the Leyden-jar; the "electric aura," from a pointed wire; and the so-called "electric bath," were the methods employed. M. Jallabert (1748) published a cure of paralysis with atrophic muscles, affecting the right arm. He recognised as results of electric stimulation—acceleration of the pulse, increase of temperature, involuntary contraction of the muscles, and emmenagogue properties.

M. Jallabert's method was the application of sparks to points of the skin corresponding to the attachment of muscles; and in the Encyclopædia of Diderot and d'Alembert (1777) this mode of exhibition is considered to account for Jallabert's success. M. l'Abbé Saus (1772) related several cases of hemiplegia, and considered electricity the only means calculated to effect a cure in paralysis of long duration.

In 1780, M. l'Abbé Bertholon published an elaborate treatise upon the electricity of the body in health and disease, in which those conditions are referred to its equilibrium on the one hand, or to some variable kind of inequality on the other. Every sort of malady is, according to this learned Abbé, amenable to electric treatment, if the physician would only find out the form of electricity required.

We view with irresistible distrust the statements of a man, who treats successfully with the same remedy all classes of disease; and finding that the Abbé Bertholon cured affections of the skin, fevers, inflammations, cachexiæ, convulsions, and paralyses with almost equal rapidity and ease, we pass on to the less ambitious but more careful memoir of M. Mazars de Cazèles. In this are related the cures of twenty cases, consisting of chronic rheumatic affections, hemiplegia, general paralyses, and neuralgiæ. In a second memoir by the same author, similar reports are furnished of forty-six cases; and M. Sigaud de la Fond confirms their general truthfulness by the detail of his own success in analogous affections. The latter author sums up the *modus operandi* of electricity by affirming that it augments transpiration, saliva, and the alvine secretions, causes abundant deposit in the urine, increases the temperature of the body, and improves its nutrition. In 1785, Cavallo published a general treatise, collecting the observations of previous experimentalists, and bearing his own testimony to the value of electrical applications for the increase of secretion generally, and in the treatment of skin diseases.

After the discoveries of Galvani and Volta, Valli was the first to apply

the new form of electricity (galvanism, or contact electricity), for the restoration of those who were in trance, or apparently dead from other causes. Sömmering pointed out the region of the phrenic nerve as the most advantageous spot for its application with this end in view. Pfaff, Reil, Humboldt, Aldini, and others announced the utility of galvanism in the treatment of paralyses of motility; Schaub, Eschke, and others, in loss of sensibility; and Sarlandière, by means of electro-puncture, made its application possible to deeper organs. Since Professor Faraday's discovery of induction-electricity, the older methods of electrifying have been almost entirely discarded, and it is with this mode of operation, now commonly called "Faradisation," that Duchenne and others have accomplished their results. There are, then, three generic forms of electricity, which may each be applied in different manners; and Duchenne has the following comments upon their relative value:

The action of *electricity* by *sparks* is always limited to the skin, or to some superficial muscle. The Leyden phial causes energetic contraction of the muscles, and some commotion of the nervous centres, with a painful feeling of shock, or of contused nerve. The *galvanic* current may be either interrupted or continuous; the latter acting only upon the skin, and producing, in proportion to its strength, either simple erythema, or an eschar. The *induction* current is, from its very nature, of momentary existence only, and, although the rapidity with which its intermittences take place may be increased or diminished at will, a continuous stream is impossible. A current of the first order acts directly upon the muscular contractility; the secondary current excites powerfully the retina and cutaneous sensibility.

The *therapeutic value* of these three forms is various. Electric sparks afford a convenient mode of stimulating the skin. They are too feeble for action upon the muscles, and the Leyden phial produces too great commotion of the central organs. The continuous current from galvanism is of service when it is desirable to obtain thermic or chemical effects; and it is the best form of application to the retina. For action upon the muscles, the induction-apparatus is the best. Its special value appears to be, that its intensity can be regulated with great precision; that it can be directed upon any organ, and limited to that organ; that it exerts no appreciable chemical action on the tissues, and leaves no textural change; that it causes less burning sensation on the skin, causes and leaves less pain in the muscles, and rarely induces disturbance of the nervous centres. Duchenne's apparatus consists of an induction-machine attached to one of Bunsen's carbon-batteries; and his special improvements are the "Graduator," a well-adjusted arrangement, by which the force of the current may be increased or diminished at will; the "Rheometer," which enables the operator to measure the force he is employing, and consequently to enhance the value of the Graduator; and the "Rheophors," or handles for applying the current. The whole apparatus is neatly arranged in a case, and may be procured of Charrière, Rue de l'Ecole de Médecine, No. 6, for 160 francs.

With regard to the *mode* of *applying* the conductors, Duchenne has made some important observations. If they are dried and placed upon the dry cuticle, not far from one another, the action is confined to the

surface. If both cuticle and conductor are well moistened (his own method being the attachment of wet sponge to the ends of the conductors) there is no action upon the skin, but marked contraction of the underlying muscles, accompanied by a very peculiar sensation like that caused by placing the conductors (through a wound in the cutis) upon the muscles themselves. The muscles may be reached, directly, by placing the moistened conductors over them; or indirectly, by placing the conductors over the trunk of their nerve. The latter method is not so precise as the former, but is of use when anaesthesia is to be combated. The sensitiveness of the skin varies widely in different situations, and the same is true not only of the sensibility, but also of the contractility of the muscles. Hence, Duchenne observes, it is necessary to become acquainted with these differences in order to administer the proper dose to a particular muscle, or group of muscles.

Some individuals are much more influenced than others, and unpleasant disturbance of the nervous centres may be induced, such as vertigo, confusion of thought, &c., without any local sensations from the application.

The *purposes* for which Faradisation may be employed are the advance of physiology, pathology, and therapeutics. The great utility of this agent in *physiology* is in establishing the functions of particular muscles and nerves. Every voluntary movement of a muscle, or group of muscles, is complicated by the involuntary contraction of their antagonists, a provisional association to insure firmness and stability in the limb; and farther, it is impossible to determine the contraction of any particular muscle by a direct effort of the will, we can determine only the kind of movement, which shall be the "end" or resultant of muscular combinations, and the production of these combinations is the function of some part of the nervous system independent of volition. Duchenne imagines that by accurate local Faradisation many errors in our ideas with regard to the mechanical uses of particular muscles may be corrected; and this may be the case, but it must require the practised hand of M. Duchenne himself to single out deeply situated muscles, and determine their special contraction apart from those which closely surround them. Richter gives an interesting account of some conclusions to which Duchenne arrived, and the aid which he received therefrom in directing his local Faradisation for the treatment of certain local spasms and paralyses.

Some of the most interesting conclusions relate to the complicated movements of the scapula, which cannot be described without much detail, or the aid of plates. With regard to results more readily appreciable, we may mention that Duchenne considers the zygomaticus major the muscle which gives the special expression of laughing; the zygomaticus minor, that which gives the appearance of crying; that the musculus extensor digitorum communis, musculus extensor pollicis longus, and the musculus extensor digiti minimi, extend only the first phalanges of the fingers and thumb, while the abductors, adductors, interossei, and lumbricales effect extension of the second phalanx of the thumb, and of the second and third phalanges of the fingers; that the musculus flexor pollicis brevis is a flexor of the first phalanx of the thumb, but to a higher degree an extensor of its second phalanx; and that the musculus supinator radii

longus is not a supinator, but a pronator of the forearm. As to nerves, Duchenne thinks that he has made out satisfactorily the function of the chorda tympani—viz., that of conferring sensibility, and the sense of taste, upon the anterior two-thirds of the tongue.

In its relation to *pathology*, electricity appears to be of service in diagnosis and therapeutics. Neither Duchenne, Meyer, nor Guitard add anything of importance with regard to the former, although they speak very confidently, and, it must be added, somewhat contradictorily upon the subject. Dr. Marshall Hall pointed out, many years ago, that when paralysis was of such a kind as to sever muscles from the influence of the spinal cord, their irritability to stimulation from the galvanic trough was less than the irritability of muscles still in functional connexion with the cord. The use of the term "spinal" to denote this kind of paralysis led to misapprehension, and was supposed to imply disease of the cord itself; consequently, cases of ordinary paraplegia became the field for experiment. Duchenne and Meyer use the words spinal paralysis in this sense, and hence modify or contradict what they suppose to be the meaning of Dr. Hall. It is at once obvious that, although muscles may be cut off from the influence of the brain, and thus paralysed in respect of volition, by many diseases of the medulla, some portion of the cord below the seat of lesion may remain intact (or even in a state of exalted reflex activity), and that these cases are not examples of "spinal paralysis," using that term in the sense clearly defined by Dr. Hall.*

The paralysis, from *exclusion* of the spinal cord, is termed by the recent writers on the subject (we think somewhat inaptly), "traumatic;" and their conclusions upon the condition of muscular irritability thus induced agree precisely with those of Dr. Hall—viz., that it is notably diminished, or absent altogether. Meyer adds, that in these cases muscular sensibility is less diminished than contractility. With regard to the *vexata quæstio* of irritability in cerebral paralysis, there is little additional information. It has been contended, on the one hand, that irritability is relatively greater in the paralysed than the non-paralysed muscles, when they are severed from cerebral influence alone, and thus paralysed to the will; it has, on the other hand, been stated that precisely the reverse is true. The observers on each side have recognised exceptional cases to what they consider the general law, and some of the differences between them are referrible to the mode of experimentation adopted. Duchenne states that in simple cerebral paralysis there is no diminution of irritability; that, as a rule, it is normal and equal on the two sides; but that often it is greater in the paralysed than the non-paralysed muscles. Meyer, although urging that the contractility on the two sides is normal and equal, relates cases in which some muscles evinced increase of irritability; and one of Guitard's hemiplegic cases, which is referred to cerebral hæmorrhage, exhibits perfect absence of contractility. We have nothing to do at present with the theories employed to account for these phenomena; it is plain that too much difference has been observed to consider the laws of irritability and paralysis established so firmly, that electricity may be used with much advantage in diagnosis. The discrepancies probably arise from variations in the mode and time of examination, and in

* See Medico-Chir. Trans., vol. xxx. p. 207.

the cases submitted to the test. Some of the latter are readily appreciable: such as permanent and active contraction of the muscles on the one hand, their relaxation or atrophy on the other. The point of interest at present established is, that simple cerebral paralysis causes, as a rule, little or no diminution of irritability; but that spinal paralysis frequently does. This general result Duchenne and others have used with some advantage in the discrimination of a few doubtful cases. It is well known that the condition of the orbicular muscle of the eyelid may, in some cases, be made the means of distinguishing facial hemiplegia, of central origin, from paralysis of the portio dura. When the orbicularis is paralysed. there is strong probability, amounting almost to certainty, that the cause of paralysis is some injury to the seventh nerve; but the absence of orbicular paralysis by no means proves the reverse; since lesion of the portio dura, not involving the filament supplying the orbicular muscle, may present the precise characters of cerebral paralysis of the face. The diagnosis is then very difficult, but Faradisation may decide the question, which is an important one both in respect of prognosis and therapeutics, as we shall see in the sequel. The following case, recorded by Duchenne,* is an apt illustration:

"In 1846, I observed at La Charité (Salle St. Joseph, No. 13, Service de M. Bouillaud) a patient who had presented for three months partial paralysis of the right side of the face. He had awaked one morning with distortion of the features. When I examined him, the right commissure of the lips was lower than that of the opposite side, but the palpebral opening was as large on one side as the other; when he laughed, only the left side moved, drawing towards it the right cheek. He could not draw up the lips on the right side in order to whistle; his right cheek became distended, and the air escaped by a large aperture, which formed between the lips on that side. Food accumulated on the right, between the cheek and the teeth, and he had difficulty in articulating the labials. But he approximated the eyelids, contracted or raised the eyebrows, as well on the right side as on the left. I found in this patient all the apparent signs of facial hemiplegia, from a cerebral cause. But the electro-muscular examination soon dissipated all doubts, for I observed that the paralysed muscles had lost their electric contractility, a phenomenon which is always found in paralysis of the seventh pair, and which does not exist in hemiplegia of cerebral causation. Without this examination, I should have been very much embarrassed to establish a diagnosis, for the patient did not recal any exposure to a current of air. In the history nothing could be found to suspect compression of the nerve. The patient submitted to local Faradisation, and left the hospital very nearly cured some weeks afterwards (he would not attend for the completion of his treatment)."

In an obscure case of paralysis affecting the muscles of the shoulder and arm, Duchenne, from observing loss of electro-muscular contractility, inferred the existence of local injury to the nerves; and subsequent examination led to the discovery of a syphilitic exostosis, compressing certain branches of the cervical and brachial plexus.

In other forms of paralysis certain differences have been observed, but they are not sufficiently established at present to be of great utility. For example, Meyer states that in paralysis from progressive muscular atrophy, and from lead poisoning, the contractility and sensibility of the muscles are diminished, *pari passu*, with their nutrition; but that in rheumatic paralysis contractility is normal, and sensibility, though some-

* Bulletin Générale de Thérapeutique Méd. et Chir., tome xlvi. p 341.

times normal, is frequently exalted. The latter condition (exalted sensibility) is the more frequent, according to Duchenne; but Meyer adds, and quotes Froriep to support his statement, that sensibility is often diminished, and that in such cases the contractility is also less than normal. This deviation from the general rule, Meyer accounts for by the presence of exudation matter in the cellular tissue or muscles.

In lead-palsy, and in traumatic (spinal, Dr. Hall,) paralysis, contractility disappears before nutrition is affected. Duchenne adds that paralysis from lead never affects contractility in the flexor muscles of the fingers, the interossei, or supinator longus, and that this is a diagnostic mark between such palsy and traumatic paralysis from injury to the radial nerve.

Richter thus sums up the general data from which some assistance may be derived in diagnosis:—Faradisation divides paralytic affections into two groups; first, those in which irritability (or contractility) is absent; and second, those in which it is present. The first group includes lead-poisoning, progressive muscular atrophy, and traumatic paralysis; the second, cerebral, hysteric, and rheumatic. In the three forming the latter group, there are differences in respect of sensibility. It is normal in the first, absent in the second, exalted in the third.

As we have said before, it does not appear that these conclusions can be considered as established, but we have thought it right to bring them forward, in order that subsequent observation may confirm their accuracy, or correct their error.

The *therapeutical* effects of electricity have been subjected to close examination by the writers whose names are placed at the head of this article. Meyer says that electricity is of great service in the treatment of neuroses, and of diseases depending upon anomalies in the processes of secretion and excretion.

The more important facts connected with the latter class refer to amenorrhœa, and these have been discussed by Dr. Golding Bird, and reviewed in an earlier number of this journal,* so that we shall not allude to them in the present article.

With regard to the former, the neuroses, Meyer speaks very favourably of the employment of electricity in neuralgia, and mentions the cure of many cases of sciatica, &c. M. Videt (quoted by Guitard), in a *Thèse Inaugurale*, 1853, relates original observations in l'Hotel Dieu de Toulouse. Of these, six are cases of neuralgia, varying from two days to three years in duration, which were all cured in from two to nine applications. Videt adds that the secondary current, and a mixed method of exhibition, were the most successful.† M. Guitard also relates six cases of neuralgia cured, or improved; three in which no amendment took place. Duchenne says that sciatica is often radically cured.

In *spasms* of various character some benefit appears to have been derived from electricity. When the contractions are clonic, the affected muscles have been subjected to excitation; when tonic, their antagonists have been stimulated to activity. Meyer states that the cause of the

* Medico-Chirurgical Review, 1849, p 373.

† By the mixed method is intended the Faradisation of the nerves and the muscles at the same time, as distinguished from the direct and indirect modes before described, p. 141.

latter class of spasm is frequently paralysis of the opposite side, and he brings forward three cases to support this view, and to prove the efficacy of Faradisation. Guitard's observations show little in favour of this mode of treatment.

After looking carefully through the statements made with regard to these two groups (hyperæsthesia and hypercinesis) we feel compelled to say, that Faradisation appears of only doubtful value in their treatment, for it is quite impossible to form a diagnosis of the nature of disease from the records which are given; the results have been but partially successful; and the diseases are well known to disappear under various forms of treatment, differing most widely from each other in all that we know of therapeutic action. With regard to the opposite conditions of disease, there is more satisfactory information, and we notice first:

Anæsthesia.—When of peripheric origin, or when the result of rheumatism or hysteria, Meyer says Faradisation is a most important mode of treatment; and also when of centric origin, such as extravasation, &c., at the base of the brain, there is evidence to show that electricity may recal the function of the nerves, provided that the extravasation, &c., have been removed or lessened. This was first shown by Steinrück, and subsequently demonstrated by M. Brown Séquard. Meyer relates three cases of anæsthesia which were cured by Faradisation. One of these implicated the branches of the fifth nerve, and the posterior divisions of the four upper cervical nerves; the other two were of the ulnar nerve. Guitard and Duchenne relate cases of amaurosis cured by electric treatment.

Paralysis.—This class of diseases has afforded the widest and richest field for the experimenter with electricity. We have already shown that the older observers met with many successful cases; and in the present day it is extremely common to find some form of electric stimulation recommended for the paralytic patient. Duchenne asks, "Is it reasonable to use local Faradisation in the treatment of paralysis consecutive to cerebral hæmorrhage?" In reply, he states that the fact is incontrovertible, that at certain periods some cerebral paralyses are cured, or improved, by purely local treatment. The following case is an instructive example:—

"La Charité, Salle St. Felix, No. 10, Service de M. Andral. Paralysis of the upper limb, stationary in a man æt. 32, and in whom a cerebral hæmorrhage had produced, ten months previously, complete hemiplegia of the right side. State of the patient before treatment by Faradism: the lower limb is no longer paralysed, the upper limb falling against the side of the body, can be moved only slightly from the trunk, by contraction of the deltoid; flexion of the fore-arm is impossible, the hand is deprived of motion, is of violet colour, and lower temperature, it has little sensibility, and has lost almost all sense of touch. After a few applications of local Faradisation, the patient could raise the arm to a right angle from the trunk, carry the hand to the head, neck, and back; flexion of the fore-arm, its extension, pronation, and supination became easy; power returned rapidly. At an advanced period of the treatment, flexion and extension of the fingers were performed with difficulty, none of the little muscles of the hand contracted voluntarily; it was only after a considerable number of applications that these muscles were cured of their paralysis. . . . . . . Subsequently, the colour, temperature, and sensibility were perfectly restored."

In commenting upon this case, Duchenne remarks with regard to the

muscles of the hand, that each of these little muscles was successively Faradised, and that after this they recovered their movements and agility. But in all cases there is not this success; in some there is perfect cure, in others partial improvement, in a third group there is not the slightest benefit. Duchenne has endeavoured to discover the cause of these different results; to establish their diagnosis, and base thereupon, not only the prognosis, but their treatment.

In 1852, M. Debout* gave the following *résumé*. Duchenne finds two periods of paralysis. In the first, the paralysis is symptomatic—i.e., dependent upon the central injury; in the second, it is idiopathic—i.e., dependent upon the muscles. In the latter case, the paralysis is localised, and this it becomes when the organ of innervation recovers its functions, the muscular fibre remaining paralysed by the simple fact of habit. The confusion of these two states has necessarily led to great mistakes in treatment. Many injurious results have followed from Faradisation during the earlier period, and Duchenne says that it should not be attempted for at least six months after an apoplectic seizure. In the later papers of this author, examples are given of injurious effects from Faradisation, when attempted in recent cases, at La Charité and l'Hôtel-Dieu. It is also urged that such treatment should not be adopted when any signs of spontaneous improvement present themselves, as the physician may then reap honours due to nature alone. It is only after six months' duration, and when the paralysis is stationary, that Duchenne thinks it right to employ Faradisation. When, however, this period has arrived, success is variable. In one-twentieth there was absolute cure, in one-fourth there was improvement, in three-fourths there was none. These different results are to be referred to the nature and present condition of the central lesion. When treatment has been successful, it is inferred that the original lesion was removed, and consequently that the paralysis was purely muscular; when only partially beneficial, that there is still some paralysis directly dependent upon the lesion or its effects, and that over and above this, there was muscular paralysis, which the local Faradisation has removed; but that when no good results, the paralysis is entirely symptomatic, or dependent upon the original lesion. Duchenne admits that it is impossible to form an accurate diagnosis of these conditions, but gives, as the result of his experience, that those patients who, after several months, present paralysis without the least muscular spasm, are readily cured by local Faradisation; while, on the other hand, those in whom spasm is observed derive no apparent benefit. He concludes that the former are instances of localised, or muscular paralysis; and the latter, of paralysis depending upon persistent central lesion: and these conclusions he has many times verified by post-mortem examination.

Duchenne endeavours to explain this relationship by the excess of spinal activity resulting from defective action of the brain; an excess which, of course, does not exist when the brain has recovered its functions. The degree of reflex activity may be considered (according to this author) an index of the amount of cerebral injury. We cannot agree with this dogma; active contractions are frequently present, when no excess of spinal reflection can be demonstrated by irritation of the skin;

* Bulletin Générale de Thérapeutique, p. 97.

yet in these cases the muscles are extremely sensitive to percussion. The whole question is involved in considerable obscurity, and is not to be decided from a few cases only; the important fact to be noticed now is, that slight contractions of the muscles do not preclude the hope of benefit from local Faradisation. Duchenne relates a case in illustration of this position, the prominent points of which are the following:—A woman, æt. 62, suffered an apoplectic seizure, leaving complete right hemiplegia, with paralysis of the tongue, involuntary passage of fæces and urine, and spasm of the muscles in the fore-arm and hand. These phenomena had been stationary for six months, when local Faradisation was first practised; and this proved eventually perfectly successful, the patient being completely cured. She was at the time under the care of M. Andral, at La Charité.

Permanent contraction of the muscles is considered by Duchenne to afford evidence of inflammatory action in the brain, which contraindicates the employment of Faradisation. This condition is to be distinguished, however, from that of simple rigidity of the flexors, arising from their persistence in a state of passive contraction. The attempt to extend the limbs in the former instance is acutely painful, and increases the spasm; in the latter it is much less painful, and will, if commenced early enough, and if cautiously maintained, remove the contraction. Faradisation, if employed in the former case, leads to very injurious results; if in the latter, there is no danger.

Duchenne finds that in facial paralysis of cerebral origin much good may be effected by electric treatment, but that the patient is thereby exposed to danger of a renewed attack. In ten cases of this description, serious accidents occurred to three; and Duchenne states, that notwithstanding the knowledge which he now possesses of the degree of excitability enjoyed by every muscle of the face, so that be can administer the precise dose which they require, it is never without hesitation and fear that he undertakes their treatment. There is no danger, however, when the facial paralysis is from injury to the portio dura; hence the importance of diagnosis, previously pointed out.

The last paper by Duchenne is concluded by some practical directions. The first of these is, that as the occurrence of one apoplectic seizure indicates some probability that another may take place, it is desirable to avoid all excitation of the central organs; and hence Faradisation should be limited as closely as possible to the muscles affected, and only a short current allowed to pass through the limbs. Farther, the intermittences should be long, for by this means there is little sensation of pain, although the current may be powerful; and at the same time with this stronger power it is possible to reach the deeper muscles. Rapid currents are too painful to be employed with anything but a low power, and then only the superficial muscles are affected.

The second general direction is, that it is necessary to act individually upon all the muscles that are paralysed. The third, that when there is spasm present in a group of muscles, it is by Faradisation of their antagonists that this treatment is of service. It is an important phenomenon, that this method of operating causes immediate cessation of the spasm, which relief continues for a certain time after the application. Lastly,

Duchenne remarks, that the "*séances*" ought not to be too long, and that if, after fifteen or twenty applications, there is no improvement, there is little hope of success.

In M. Guitard's treatise are given the records of four cases of hemiplegia, treated by Faradisation, and three of these with some measure of success. The first of them presented complete paralysis of motion and sensation in the arm, but incomplete in the leg. The duration of paralysis was two years. Hand and fingers rigidly flexed; wrists pronated. Treatment was continued, more or less regularly, for nine months, and at the end of that time there was great improvement; the fingers could be voluntarily extended, and the arm was useful. At the first, there was no contractility observed in the muscles, but it returned after the third application. The diagnosis in this case was hæmorrhagic apoplexy, but it is somewhat at variance with other statements made by these electricians, that the electro-muscular irritability was absent.

The second case is one in which hæmorrhage was diagnosticated. The patient, a man æt. 72, presented complete hemiplegia of two months' and a half duration: there was marked muscular atrophy. After treatment for one month, there was some improvement, the fingers and arm being capable of executing voluntary movements.

The third case is one of hemiplegia in a woman æt. 23. Electric treatment was only attempted twice, and without result.

The fourth case is of a complete hemiplegic attack in a chlorotic female æt. 18. The leg recovered in eleven weeks, but imperfect paralysis remained in the arm. After two Faradisations, the catamenia appeared (for the first time); and by subsequent treatment the cure was established. M. Guitard, in the same treatise, refers to numerous cases of paralysis treated with success by recent observers; but of those which he has recorded from his own experience, there cannot be any very satisfactory conclusion established. The last case is one in which the diagnosis of cerebral lesion is imperfect; the third shows nothing in either direction; the second is probably one of the cases in which Duchenne would think the honours due to the *vis medicatrix naturæ;* and the first relates "improvement" only, after nine months' treatment. But this first case, and the results of M. Duchenne's treatment, and those with which we are familiar in our own country, prove that many cases, even of long duration, are amenable to treatment. It is erroneous to suppose that the only way in which Faradisation acts is by inducing contraction in, and thus improving nutrition of, the muscles; for in many cases no contraction was induced until after repeated applications. The explanation given by Duchenne is the following:

"Le mouvement volontaire repose sur une *trame organique* soumise à une double influence, *l'innervation,* et *l'hématose.* Comment expliquer autrement les paralysies chlorotiques, saturnines, etc.? C'est donc à leur point de conflit qu'il faut s'addresser pour ranimer les propriétés vitales. . . . . Le *mouvement curateur* se produit au *conflit du sang et de l'influx nerveux avec la fibre musculaire.*" (Guitard, p. 194.)

Whether this is a correct account of the process or not, it is important to remember the fact upon which it is based—that those cases are not incurable in which contractility appears to have become extinct. The order in which improvement takes place is said to be the following:

The return of (1) electro-muscular sensibility; (2) Warmth of the skin; (3) Improved nutrition; (4) Electro-muscular contractility; (5) Obedience to volitional stimulus.

Meyer concludes, from his own observations, that the duration of paralysis exercises little or no influence upon its prognosis, but that the age of the individual affected does; the greater the age, the less is the probability of cure.

Paraplegia.—M. Guitard refers to five cases under the care of MM. Fouquier, Roux, Leroux, James, and Polin, all of which were successfully treated by electricity, but of course there is no evidence to show that, in any one of these, there was organic lesion of the spinal cord. M. Guitard relates two cases occurring in his own practice, in one of which (incomplete at the commencement) there was some improvement; in the other, marked amelioration for a time, but subsequent aggravation of the disease, which necessitated the discontinuance of Faradisation.

In those very interesting but obscure forms of paralysis which occur as the direct or remote consequences of certain blood-conditions, such as *rheumatism, lead-poisoning,* &c., Faradisation has proved of immense service. M. Guitard relates two cases of rheumatic paralysis, the first of which existed in a most aggravated form. The patient was twenty-three years of age, and had suffered for three years; there was general emaciation, and an emprosthotonic condition, the head being drawn forwards on the chest, the thighs flexed upon the abdomen, and the legs upon the thighs. After Faradisation for one month, the head could be moved into an erect position, and towards either side; the legs could be moved into and out of the bed. Treatment was neglected for a month, and all the worst symptoms returned: it was recontinued for six weeks, and at the end of that time restoration was almost complete. The second case was of rather less severity, but of the same character as the first; and after treatment for one week, many voluntary movements could be executed with facility. Although electro-muscular sensibility was preserved, contractility was lost (another observation opposed to the general statements of Meyer and Duchenne). The first and second applications produced abundant diaphoresis, after which there was notable improvement; the patient became weary of the applications and their painfulness, or it was hoped that much greater improvement would have ensued.

In *General Progressive Paralysis,* whether idiopathic or complicating insanity of mind, electric treatment has proved of service. M. Briere de Boismont asserts that these cases may be diagnosticated by means of the electric current; contractility being absent in the former, but retained in the latter. To support the truth of this statement, nine cases are recorded. M. Duchenne relates the following cases of paralysis from progressive muscular atrophy: In the first, the serratus magnus of the right side, the trapezius, rhomboid, and other muscles of the shoulder and trunk, were affected, and fibrillar contractions occurred in many regions of the body. The second case was one of far-advanced general atrophy. The third was limited to the sacro-spinalis, deltoid, trapezius, and some small muscles of the hand. Each case was completely cured.

On Traumatic Paralysis.—M. Guitard quotes the following remarks from M. Debout: The excitation of a muscle takes place only in those

points which are *en rapport* with the excitors; the latter ought, therefore, to be moved successively over the whole surface of the muscles. The intensity of the current should be in direct proportion to the thickness of the muscles: the rapidity of intermittences in inverse proportion to their contractility. In traumatic paralysis, there is nothing to fear from central disturbance. Duchenne remarks, it has been too generally considered that the paralysis which occurs from lesion to a nerve is incurable, since local Faradisation frequently effects their cure. Cases are recorded in support of this assertion. Dislocation of the shoulder downwards produced paralysis in two instances, and the cure was complete in each. In a third case, there was paralysis with atrophy of the muscles in the hand, the result of laceration of the radial nerve four years previously. The fingers had assumed a vicious position. Under the influence of local Faradisation, there was the successive return of—1. Warmth; 2. Nutrition; 3. Tone; 4. Position of the fingers; and 5. Volitional movement; without the production of a single electric contraction during the whole course of treatment.

Paralysis arising from compression of the muscles themselves, or from distension of their fibres, has proved amenable to the same therapeutic agent. Duchenne remarks that, when muscles are thus paralysed, they retain their electro-contractility, which may serve to diagnosticate the case from one of injury to the nerve.

That form of immobility, in respect of volition, which is accompanied by tonic spasm of the muscles, has been frequently cured through Faradisation of the antagonistic muscles. Thus, M. Debout relates a case of torticollis posterior, due to contraction of the rhomboid muscle, and the levator anguli scapulæ cured by three applications of electricity to the serratus magnus. M. Duchenne records another case of torticollis of the clavicular portion of the trapezius, cured by Faradisation of the trapezius on the opposite side; also, an instance of distorted face, from tonic spasm of the zygomaticus minor, cured by causing contraction in the muscle of the opposite side. M. Guitard relates the cure of similar deformities.

In the September number of Schmidt's 'Jahrbuch' are, detailed by Krug, eleven cases of impotentia virilis, cured by electricity. This account is the abstract of a paper (in the Vienna 'Wochenschrift') by Schulz. The cause of impotence was, in each case, the absence of erection, due to exhaustion from past excesses of various kinds.

Although some of the records which we have extracted from the several treatises under review, and many others which our space will not permit us to relate, were followed by an almost fabulous success, there can be no doubt that electricity, in its recent mode of application, has been of greater service than many in this sceptical age were willing to admit. Upon the employment of this agent in surgical and obstetric practice, it is not our intention to enter. In the former, and, to a certain extent, in the latter, the object for which it is used, and the mode of its application, are so different from those of the affections which we are now considering, that they require a more special notice. The class of disease which is more or less amenable to treatment is, as is well known, the paralytic. We do not consider that the recent writers upon this subject have added much to the information which we possessed when Dr. Bird's lectures were deli-

vered at the College of Physicians, in 1847. In respect of diagnosis, there is some confirmation of what was almost as certain before; but we derive little or no fresh assistance. With regard to treatment, we find only the old belief established—viz., that where paralysis persists from habit, after the removal of some central lesion, or of some modified condition of the blood, electricity is of great service in re-awakening the dormant muscular function. It is, in the present state of therapeutical science, the only means by which we can arrive at these inactive muscles; but when the paralysis depends upon some persistent disease, or upon some disorganisation of the nervous centres, electricity is of no service to the muscles, it can effect no removal of the paralysing cause, and it may be highly injurious to the individual in various ways.

What it does appear that MM. Duchenne, Guitard, and others, have shown is—1. That local treatment—i.e., Faradisation of the muscles individually—is the safest and most effectual mode of proceeding; 2. That it is possible, by a practised adaptation of the apparatus, to accomplish this individual excitation; 3. That such treatment should not be discouraged, because the muscles exhibit at the first no contractility; 4. That it should be continued in spite of apparent failure, provided no evil effects result.

The records of successful treatment by electricity of numerous other diseases—such as epilepsy, cholera, angina pectoris, coughs, ascites, &c., &c., make little addition to the sum of our previous knowledge—viz., that all of these affections may occasionally yield to this or any other form of medication, without our being able to account for the results upon any received principle of pathology or therapeutics.

The cases of this kind enumerated by Guitard and others need farther confirmation to call for special consideration.

The researches of M. Duchenne have hitherto appeared scattered through various journals, and extending over some length of time; but he has promised—and there is "*actuellement sous presse*"—a general account of his works, in the form of a 'Traité de la Faradisation localisée, et de ses Applications à la Physiologie, à la Pathologie, et à la Thérapeutique,' and it is with much pleasure that we anticipate its appearance.

J. Russell Reynolds.

REVIEW XIII.

Census of Ireland for the Year 1851. Part III. *Report on the Status of Disease.* By W. DONNELLY and W. R. WILDE. Presented to Parliament. Folio. 1854.

WHEN the arrangements were in progress for taking the census of Ireland, on the night of the 30th March, 1851, it appeared to the Commissioners that some valuable information regarding the sanitary condition of the country might be gained, by procuring returns of all persons labouring under disease of any kind, either at their homes or in public institutions. This was accordingly done, and forms the groundwork of the Report now before us. The statements regarding some of the diseases were subsequently verified, and more detailed information concerning the subjects of them obtained, through the medium of the constabulary force.

The results cannot be considered as representing the average health of Ireland, for they merely show the number sick, and their diseases on a given day; and there are no means of ascertaining whether sickness was more or less prevalent than usual at the time. They are also open to the objection, that although the returns from public institutions are probably accurate, those of the sick at their own homes must be deemed to be merely approximations to the truth. As, however, the sources of error must be nearly the same over the whole country, the results may be relied on as showing the relative prevalence of the various classes of diseases in the different Provinces and Counties. Much valuable and interesting information has been collected on the subject of certain specific affections, particularly deaf-dumbness and blindness, to both of which conditions Mr. Wilde had long directed his attention.

The diseases in the general returns were divided into two classes:—permanent and temporary maladies.

"Among the former may be classed the deaf and dumb, the blind, the lunatic, idiotic, paralytic, and epileptic, as also the lame and decrepit; while under the latter head may be placed all those labouring under the ordinary acute and chronic maladies to which the inhabitants of this country are liable."

In his remarks on the "Permanent Diseases," Mr. Wilde has given a very interesting outline of the history of the instruction of the deaf and dumb from the earliest periods, and of the institutions founded in Ireland for that purpose. He has given a similar account of the asylums for the blind, and of those for lunatics and idiots. In the report upon the number and condition of the sick in the public hospitals, he has traced the history of these establishments from the time when "stood the *Broin Beary*, or the 'House of Sorrow,' where the sick and wounded were provided for," down to the epidemic years of 1847-8, when many temporary hospitals were erected for the treatment of the sick poor. Into these subjects, however, we cannot enter, nor can we afford space for the consideration of many interesting details respecting the condition of the permanent sick, such as the sexes, ages, occupations, and social condition of the deaf and dumb, the causes of congenital and acquired muteism, and the extent of education among this class; or the numerous facts recorded concerning the blind and the insane. Much of the information is given in a tabular form, and is so condensed already as to be difficult of further abridgment. Recommending the Report, therefore, as well worthy of careful study by all who are interested in the subjects treated, we shall content ourselves with stating a few of the general results as to the amount of these diseases, and which, for convenience, we have condensed into the following table:

	Males.	Females.	Total.	Proportion to population.	Relative proportion of sexes. Males.	Females.
Deaf and dumb	2688	2059	4747	1 in 1380	100	74·5
Blind	3588	3999	7587	1 in 864	100	111·5
Lunatics	2503	2571	5074	1 in 1291	100	102·3
Idiots	2666	2240	4906	1 in 1336	100	84·0

For the purpose of comparison, the following statement, compiled from the population tables of Great Britain, is submitted, showing the proportion of deaf mutes and blind in England and Scotland respectively. The

results as regard lunatics and idiots are not given, because in the Irish Report, all of that class, whether at large or in confinement, have been included, while, in the English and Scotch returns, none have been enumerated but those in public and private establishments for the insane.

		Proportion to population.	Relative proportion of sexes.	
			Males.	Females.
Deaf and dumb	England and Wales	1 in 1738	100	82·9
	Scotland	1 in 1340	100	80·0
Blind	England and Wales	1 in 979	100	88·4
	Scotland	1 in 960	100	105·3

The most striking facts elicited by this comparison are the greater number of deaf-mutes in Scotland and Ireland than in England; the higher proportion of blind in Ireland; and the difference in the relative proportion of males and females affected with blindness, being in England, 88, in Scotland, 105, and in Ireland, 111 of the latter to 100 of the former. In the Great Britain Report, this excess in Scotland is said to be "a result probably traceable to the preponderance of aged women in that country." We know not whether the same preponderance exists in Ireland, but it is worthy of remark, that the excess of blind females there is entirely among those above forty years of age. The greater number of blind in proportion to the population, is probably the result of the epidemics of ophthalmia which have occasionally prevailed in Ireland, and the last of which extended over the three years 1849-51. During that period, 86,959 cases of the disease were treated in the Irish workhouses; and on the day on which the census was taken, there were 3457 persons affected with it in these establishments.

Passing from the consideration of these diseases to the general returns, including all cases, whether belonging to the class of permanent or temporary maladies, we find that the number sick, at their own homes or in public institutions, on the night of the 30th of March, 1851, amounted to 104,495, being 1 in 63 of the whole population. The proportion, however, varied materially in the different provincial divisions of the country. Thus, it was 1 in 46 in Munster, 1 in $58\frac{1}{2}$ in Leinster, 1 in 74 in Connaught, and only 1 in 92 in Ulster. This striking difference appears to be intimately connected with the social condition of the people. If we divide them into three classes—1st. The paupers in the workhouse; 2nd. The inmates of the various prisons; and 3rd. The remainder of the population—we find that, of the first class, 1 is sick in every $5\frac{3}{10}$; of the second, 1 in 15; and of the third, only 1 in 112, although in the last are included all the hospitals and asylums, except those attached to the prisons and workhouses. It follows, therefore, that if there be a great preponderance of paupers in any Province, the proportion sick will stand relatively high. According to the data in the Report before us, the proportion of paupers in the workhouses at the time of taking the census was, in Munster, 1 in $13\frac{8}{10}$ of the population of the Province, in Connaught, 1 in $24\frac{4}{10}$; in Leinster, 1 in $32\frac{7}{10}$; and in Ulster, 1 in $84\frac{7}{10}$. From this it will be clearly seen how much the general results must have been influenced by this circumstance. The amount of sickness in Leinster is higher than in Connaught, although the proportion of pauper population

is less; but this difference may probably arise from the number of cities and crowded towns in the former, and the numerous hospitals and asylums for the sick in Dublin.

In the appendix to the Report, detailed abstracts are given of "the number, sexes, and diseases of the sick at their own homes, or in public institutions," in each of the Provinces on the night of the census. From these we have compiled the following Table, showing the proportion sick by each class of diseases in every 10,000 of the population:

	Leinster.	Munster.	Ulster.	Connaught.	Ireland generally.
Zymotic, or epidemic, endemic, and contagious diseases	47·8	94·3	23·1	47·9	53·4
Sporadic diseases					
Diseases of the brain, nervous system, and organs of sense	44·3	37·3	36·1	29·0	37·4
Diseases of the circulating organs	1·1	0·9	0·7	0·6	0·8
Diseases of the respiratory organs	18·1	20·6	12·3	11·6	16·0
Diseases of the digestive organs	8·3	7·1	6·0	5·9	6·9
Diseases of the urinary organs	0·7	0·3	0·3	0·5	0·4
Diseases of the generative organs	1·7	1·0	0·7	0·9	1·0
Diseases of the locomotive organs	15·9	15·5	10·9	10·7	13·5
Diseases of the tegumentary organs	12·2	16·1	4·2	12·9	10·9
Diseases of uncertain seat	18·0	20·2	11·6	12·6	15·9
Accidental causes	1·9	1·8	2·0	1·6	1·9
Causes not specified	1·4	1·8	0·9	0·8	1·3
Total by all causes	171·4	216·9	108·8	135·0	159·4

An examination of this Table shows that, with the single exception of accidents, the prevalence of all the classes of diseases is, in Ulster, below the average of Ireland generally, but that the exemption is most striking in the zymotic diseases, and in those of the respiratory and tegumentary organs. Connaught enjoys a like exemption from pulmonary affections, and the diseases of the brain and nervous system are lower in it than in any of the other provinces. Zymotic diseases were four times as prevalent in Munster as in Ulster, and twice as prevalent as in the other two provinces. The reporters observe that they were most numerous

"In the city of Kilkenny and the counties of Clare and Kerry, the city of Waterford and the town of Galway, in which localities the proportion varied from 1 in 55 to 1 in 94 of the population; and least in the counties of Antrim, Down, Armagh, Donegal, and Dublin, and also Belfast town; showing, in the former instance, the effects of poverty and destitution in the production and maintenance of epidemic diseases; and in the latter, those of comfort, industry, and cleanliness in maintaining a comparative immunity from diseases of an epidemic or contagious character."

Fever was the most common disease of this class, constituting, indeed, one-eighth of the whole number sick, and upwards of one half of the cases were in the workhouses and the workhouse hospitals. Dysentery and diarrhœa were next in point of numbers; "they usually follow in the track of fevers;" and, like them, are chiefly to be found in the workhouses. An investigation seems imperatively called for on the part of the authorities into the causes of fever being so very prevalent in the workhouses, and into the best means of reducing its amount. They are, for the most part, large and handsome buildings, not overcrowded, and remarkably clean, and yet fever appears to prevail to a great extent in them. It is worthy of note, that while in Munster two-thirds, and in

Leinster three-fifths, of the fever cases were in the workhouses, in Ulster the proportion in these establishments amounted to little more than one-fourth. Again, the ratio which the cases of fever bear to the inmates of the workhouses differs greatly, being in Leinster 40, in Munster 34, in Ulster 25, and in Connaught 17½ per 1000. Here, then, is a field for investigation; what differences exist in the construction and in the management of these establishments, and what influence are they likely to exert on the health of the inmates?

Influenza and ophthalmia were the other prevalent diseases of the zymotic class; the latter was most rife in Cork, where as many as 1 in 50 of the population were affected; and it was also very common in Tipperary, and in the county and city of Limerick.

Next to the zymotic, the most prevalent diseases were those of "the brain, nervous system, and organs of sense," consisting chiefly of blindness, insanity, idiocy, deaf-dumbness, and paralysis. These, as already stated, have been treated of separately under the head of permanent diseases, and we must refer our readers to the Report itself for the full details concerning them. The reporters observe: "It is gratifying to find that so few cases as 9 were returned under the head of delirium tremens;" a remark in which we should most cordially concur, if we could only persuade ourselves that no cases had been omitted, or returned under a different name.

The next diseases in point of frequency are those of the respiratory organs, furnishing a tenth of the whole amount of sick. They are considerably more prevalent in the south and east Provinces than in the north and west. About two-fifths of the cases were consumption, which appears to be much more common in Leinster and Munster than in Ulster and Connaught; the proportions being 79 and 68 in 10,000 of the population in the former, and 55 and 49 in the latter Provinces.

Diseases of the locomotive organs form a considerable item in the table. Nearly one-half of the cases were rheumatism, and above a fourth were returned as "lameness." On the day of the Census, 519 persons were labouring under fracture, 75 under dislocation, and 58 under the effects of amputation of some of the extremities.

The only other class to which we shall advert is that of diseases of the tegumentary organs, from which Ulster enjoys a very marked exemption, the ratio being only one-third as high as in Leinster and Connaught, and one-fourth as high as in Munster. The difference arises chiefly from the almost entire absence of itch, and also from the low proportion of cases of scald-head and ulcers.

We must now conclude these remarks, referring our readers for further details to the Report itself. Mr. Wilde is entitled to great credit for the industry with which he has collected the facts, the clearness with which he has arranged them, and the interest he has contrived to throw into the usually dry details of a statistical report.

REVIEW XIV.

1. *De cognoscendis et curandis Placentæ Morbis.* WILDE.—*Berol.*, 1833.
2. *Handbuch der Speciellen Pathologischen Anatomie.* Von CARL ROKITANSKY.—*Wien*, 1842.
3. *Lectures on the Theory and Practice of Midwifery.* BY ROBERT LEE, M.D.—1844.
4. *Ueber die Krankheiten des Eies und der Placenta.* GIERSE und H. MECKEL. ('Verhandl. der Ges. für Geburtsk.')—*Berlin*, 1847.
5. *Abhandlungen über den Bau der Molen.* Von H. MÜLLER—*Würzburg*, 1847.
6. *Ueber den Bau der Molen.* METTENHEIMER. ('MULLER'S Archiv,' 1850.)
7. *Note sur les Altérations du Placenta.* Par CHARLES ROBIN. ('Archives Gén. de Méd.,' Juin, 1854.)*

(*Continued from No. 27, p.* 36.)

IN the writings of the older obstetric authors there occur isolated reports of cases in which morbid conditions of the placenta were observed. Many of these cases are of great interest, as illustrations of the general fact of a close connexion between morbid alterations of the placenta and abortion or death of the fœtus. But very few, if we except the instances of that remarkable and unmistakable condition known as the vesicular mole, are of any value as illustrations of the morbid anatomy of the organ. We have already stated our opinion, that it is owing to the want of that minute microscopical analysis of the altered structures, without which an accurate knowledge of structural alterations cannot be obtained, that the greater part of the cases recorded by the older authors must be rejected as useless in any attempt to delineate the history of the diseases of the placenta. What would it avail to discuss the pathological import of morbid appearances, the real nature of which is uncertain? They defy all attempts at interpretation. In thus excluding all those cases which are defective from imperfect investigation, or obscured by erroneous interpretation, we are not insensible to the attending advantage of greatly simplifying our task by narrowing the field of our inquiry. In passing under review the facts recorded by recent observers, we shall tread upon firmer ground; and the soundness of the conclusions we may arrive at will be more easily tested by others.

From time to time various authors have sought to collect the scattered records of individual diseases into a systematic compendium of placental pathology. Some of these evince considerable research; few bring any original additions to extend or to correct what was previously known. Besides these necessarily imperfect attempts at forming systems of placental pathology, there exist innumerable records of cases and many monographs treating of particular diseases. It is not our purpose to give a bibliography of the subject, or to enter upon a minute critical analysis

* The titles of other works will be found at the head of the first part of this article.

of the more important contributions. But the plan we have proposed to ourselves would not be complete without passing in rapid review the names of the principal authors who have laboured to illustrate the subject before us.

One of the earliest systematic attempts was that of Schacher and Seiler, who published a treatise, entitled 'De Placentæ Morbis,' in Haller's 'Disputations,' (vol. iv.) in 1709. This essay contains a summary of the old opinions, but not much that deserves to arrest attention at the present day. In the edition of the works of Vallisnieri published at Padua, in 1710, there is an excellent commentary upon a case of hydatidinous placenta, entitled 'Storia del Parto Vesicolare.' In the 48th epistle of Morgagni, 'On Moles and Abortions,' are several interesting cases. Stein and D'Outrepont have each contributed important cases and observations.

But the first author whose views we think it useful to notice is Murat. This writer, in a special article on the diseases of the placenta, drawn up for the 'Dictionnaire des Sciences Médicales,' gave a systematic summary of the scattered observations of previous authors. He observed that the placenta might be altered in its colour, dimensions, structure, or consistence; that it might be scirrhous, cartilaginous, or osseous; that its adhesions might be too dense or two slender; that different concretions, hydatids, might be found; that its protracted retention in the womb might occasion different modes of alteration; and lastly, that rupture of the parenchymatous tissue had been observed. He especially observes, that the colour and substance of the placenta are often changed in women affected with syphilis; and that these affections seem to favour detachment. He says, further, that osseous or calcareous concretions have been found; sometimes true steatomatous concretions; and, occasionally, sanguineous concretions. Of all the diseases of the placenta, he says, that in which this organ is transformed into a vesicular mass is the most frequent. It cannot be said that the article of Murat possesses any greater merit than that of being the first attempt to epitomise the observations of other authors.

The contributions of M. Dance are of a more original character, and of greater interest than that of Murat. In his first paper* M. Dance relates two cases: the first is an example of inflammation of the decidua; the second, of congestion of the placenta. In another paper† M. Dance relates an example of inflammation of the chorion and amnion. These cases are all valuable, as elucidating the pathology of the ovum: they will again engage our attention.

The next author is Brachet, who describes several cases of inflammation of the placenta and membranes. He sums up his observations with the general remark, that the placenta is subject to the same affections as the other organs, and that these are produced in it by the same causes. Inflammation, he says, is the most frequent. The facts he relates constitute an important accession to the subject.

Cruveilhier, appreciating fully the great importance of the subject, added largely to our knowledge by numerous valuable cases, and by many admirable reflections. He observes, that "by the placenta, morbid causes

* Répert. Gén. de Méd., tom. iii., 1827. † Arch. Gén. de Méd., 1829.

are transmitted directly to the fœtus, the complex organization of which is susceptible of all the diseases observed in the adult; but the placenta itself may be subjected to the influence of some of these causes, and in some degree may arrest them; the channels for the transmission and revivification of the nutritive materials being interrupted wholly or in part, the child is born dead, or greatly enfeebled. It may be said that the diseases proper to the fœtus influence its nutrition only in a moderate degree, and that the diseases of the mother exercise over this nutrition a much smaller influence than do the diseases of the placenta itself." We shall adduce evidence hereafter to show that this eminent pathologist has, in this summary, somewhat underrated the influence of the diseases of the mother upon the fœtus. He thus classifies the diseases of the placenta:

1. *Hypertrophy.*—This consists sometimes in a serous infiltration, analogous to that so often observed in the umbilical cord. In one case related, this condition coincided with a pseudo-membranous infiltration.

2. *Atrophy*, which may be either general or partial, invading particular cotyledons only.

3. *Inflammation.*—M. Cruveilhier cites the observations of Brachet in proof of this affection.

4. *Ossification.*—This almost always takes place on the uterine surface. Two kinds may be distinguished: in one, there is an osseous, or rather a stony shell, one or two lines in thickness, covering uniformly, or in patches, the uterine surface, without penetrating its substance; in the other, a kind of osseous needles penetrate the placenta, and traverse it in every direction. This kind of petrification always proceeds from the uterine towards the fœtal surface. The seat of the first appears to be in the fibrous membrane which invests each cotyledon (decidua); the seat of the needle-like concretions, or of the small masses forming grit, is very obviously in the arterial vessels.

5. *Hydatidiform Cysts.*—This is the most frequent of the alterations of the placenta. Cruveilhier has the merit of demonstrating that these cysts are not hydatids.

6. *Apoplexy.*—This name is given to a condition in which collections of blood are found in the torn substance of the placenta.

We shall have occasion to recur to the facts and views adduced in the writings of Cruveilhier.

Wilde,* who next attempted a methodical arrangement of the diseases of the placenta, has been frequently, but without much reason, referred to as an authority upon the subject. He divides all diseases of the placenta into three genera: *dynamical*, *organic*, and *mechanical*. Dynamical diseases are those which affect the vital forces of the placenta. Organic diseases are those which attack and impair the structure of the organ. Mechanical comprise injuries and solutions of the relations of the organ with adjacent parts. Under the head of dynamical diseases he ranges inflammation, suppuration, hepatization and induration, and gangrene. Under organic diseases he places hypertrophy and atrophy, designating these, diseases of evolution; as diseases of *intimate cohesion*, hardness and ossification, and mollities or malacia; as diseases of the proper texture, scirrhus, and the "placenta obesa;" as diseases of hetero-

* De cognoscendis et curandis Placentæ Morbis. Berol., 1833.

logous formation, tumours. The mechanical diseases are, faults of adhesion —viz., firm and lax; faults of position, as placenta prævia and antica; wounds; faults of original conformation, as defective, double, and membranaceous placenta. Hardly any original information, either in the way of fact or deduction, is contributed in this thesis. It may be characterized as an imperfect digest of the then current ideas about placental pathology. That Wilde's own notions concerning the diseases of the placenta could not be very accurate may be inferred from the following statement concerning the physiology of the organ:—"Multi placentam distinxerunt in fetalem et uterinam, *quæ tamen differentia non existit.*" Poverty of material is concealed under the imposing garb of the Latin tongue. As a digest of the subject, it is inferior to the article of Murat.

M. Ollivier (d'Angers) relates an interesting case of inflammation of the membranes of the ovum. In 1835, Troll published an inaugural dissertation, 'De Placentæ Morbis.' It is a mere compilation.

The work of Dr. Granville supplies many valuable materials towards the pathological history of the placenta. The descriptions of this author are indeed somewhat obscured by his erroneous ideas concerning the nature of the several membranes of the ovum. He insists that the covering found external to the chorion in early ova, that is the decidua reflexa, is a proper tunic of the ovum, and calls it the "cortex ovi." The effect of adopting this error would be to render futile any attempt to connect the morbid conditions of this structure with the morbid states of the uterus and the mother. Fortunately, however, the morbid ova described are also depicted in most accurate and truly beautiful drawings. We are thus enabled to judge of the exact nature of the structures affected, and of the changes they have undergone, with almost as much certainty as if the fresh specimens were placed before us. These plates exhibit various morbid appearances of the decidua, chorion, and amnion in the early ovum; sanguineous infiltration of the decidua; hydatidiform chorion; thickening and discoloration (inflammation) of the chorion and amnion. The conditions so frequently observed in ova aborted at an early period are well illustrated; but no light is thrown upon the alterations the placenta is liable to at the more advanced periods of its development.

The elaborate essay of Professor Simpson contains a most able analysis of much that was hitherto known, and its value is greatly increased by the addition of many important original observations. The scheme of the Professor was evidently intended to embrace the entire range of the morbid alterations of the placenta, but the portion published includes only *congestion* and *inflammation.* He states that these two are

> "The most frequent and important diseased states to which the placenta is liable. . . . The placentary parenchyma, and the membranes investing the organs, are, however, liable to other morbid states—to *hypertrophy* and *atrophy*, to *softening* and *induration*, to *cartilaginous* and *calcareous degeneration*, and the secretion or formation of other morbid products and tissues, to an anormal *cystoid* or *hydatiform* structure, and to various forms of *malformation* and *displacement*; but in a practical point of view most of these lesions are comparatively less important than congestion and inflammation, and the effects which these conditions produce."

We shall have frequent occasion to recur to this essay for illustrations of the various morbid alterations of the placenta. In the clinical lecture

of Dr. Simpson, the title of which is given at the head of this article, he again refers to inflammatory induration and degeneration of the placenta, and to hypertrophy.

The account that Rokitansky* gives of the pathology of the placenta is so summary as to display most clearly the obscurity of the subject. After observing that the placenta may vary as to *size*, *shape*, and *position*, he observes that placental *hæmorrhages* may occur from violence causing separation from the uterus, or lesion of the placental structure. He says that the blood may be either infiltrated in the parenchyma, or collected into foci. The placenta may be also affected with *plethora* and *congestion*. *Inflammation*, which he says is the most frequent of placental diseases, he describes more fully. We shall revert to this part of the Professor's description hereafter. He barely alludes to the *hydatid mole;* refers to bony and calcareous depositions in a few words; denies the existence of *tuberculosis* of the placenta; exposes the misapplication of the term *scirrhosis;* and says that adhesions of the placenta to the fœtus have been observed. For minuteness and definition, the account given of the morbid alterations of the placenta, by this great pathologist, is in striking contrast with the luminous precision with which he has discussed the morbid alterations of other organs.

Dr. Lee† has, from the stores of that ample experience which has illustrated so many difficult obstetric problems, contributed many facts of interest in placental pathology. He questions the frequency of inflammation. He says that hypertrophy or atrophy, and apoplexy, are amongst the most frequent diseases. He relates in detail the histories of thirteen cases in which the placenta was found diseased. These cases are mostly striking examples of the relation frequently existing between disease of the placenta and abortion. In some instances, although no microscopic characters are given, the general description of the morbid appearances is perhaps sufficiently clear to indicate the pathological nature of the change. But upon the hazardous ground of conjectural interpretation, we have already declared our resolution not to tread.

The subject-matter of the contributions of Gierse and Meckel, H. Müller, Mettenheimer, Schrœder van der Kolk, Virchow, of those of the reviewer, and the consequent researches of Dr. Handfield Jones, Dr. Druitt, M. Ch. Robin, and Dr. Cowan, will be more conveniently discussed hereafter.

Having thus rapidly surveyed the principal writings which contain the most authentic and systematic information on the subject, we will now apply ourselves to the task of including in a methodical survey all that may be considered available and trustworthy in the construction of a comprehensive placental nosology, such as may serve to define the boundaries of our actual knowledge, and be suggestive of further advances. We have already pointed out the leading principle in such an investigation, which appears to us the most promising in useful results. 1. There are morbid conditions of the placenta which may originate in its own structure. 2. There are morbid conditions of the placenta resulting from the state of the blood of the mother brought into it, or from contact with

* Handbuch der Speciellen Pathol. Anatom., 1842.

† Lectures on the Theory and Practice of Midwifery, 1844.

diseased uterine structures. 3. There are morbid conditions secondary to disease, or defective developmental force in the embryo. In adopting this plan, we will not disguise the difficulties that must attend the attempt to adhere to it rigidly. It will often be difficult, if not impossible, to determine whether a particular lesion owes its origin to one or more of the sources referred to. But difficulties, perhaps not less grave, would attend any other plan. Some classification is essential; and we are sure we shall meet with indulgence, if we select that which seems to us to present the greatest facilities for developing our views upon so extensive and intricate a subject. We shall not hesitate to diverge, as occasion may require, from a scheme which is adopted only on account of its general convenience.

The lesions that may with the least doubt be ranged under the first division of strictly local affections are, mechanical injuries, such as rupture of the placental tissues, congestion, extravasation of blood (by some authors called apoplexy and aneurism), inflammation, hydatidiform degeneration of the chorion. Fatty degeneration is sometimes primary in the placenta, but perhaps more frequently of secondary origin. To what extent hypertrophy or atrophy are primary is doubtful. Calcareous and osseous deposits are most frequently connected with constitutional conditions of the mother, and their seat is commonly in the maternal structures.

Under the second or maternal division may be included all those conditions which may be clearly traced to abnormal states of the mother's blood. Inflammation probably arises from some such condition. We shall adduce evidence to show that one form of hypertrophy, at least, arises from disease of the mother's blood. Atrophy we believe to be more commonly dependent upon fœtal conditions. One form of fatty degeneration, that commencing in the decidual element, belongs especially to this division. That form of this affection which begins in the fœtal element, the chorion, may also, sometimes at least, be traced to a maternal cause. Calcareous and osseous deposits are almost invariably so associated. The source of fibrinous deposits may be either the maternal or the fœtal blood. The same may be said of serous or dropsical effusions.

The third or fœtal division will comprise atrophy; many cases of fatty degeneration of the chorion; some deposits of fibrinous masses; serous effusions; some forms of sanguineous congestion, and of extravasation.

I. We put aside all consideration of the mechanical lesions of the placenta, such as laceration of its structure, as scarcely coming within the definition of disease The first morbid condition we propose to consider is that which presents the least divergence from the healthy state, namely, congestion. Inasmuch as there are portions of two distinct circulating systems in the placenta, there must also be two distinct forms of placental congestion. The maternal placenta may be congested. The fœtal placenta may be congested. Strictly speaking, in the great majority of instances, each of these forms is connected with some abnormal condition of the circulatory apparatus, or of the blood, of the mother or of the fœtus. But either form, in a mixed form in which both the maternal and fœtal placentas are congested, may, under some circumstances, depend upon simply local causes. In the case of a placenta of an advanced period, it must often be difficult to determine whether the maternal or fœtal congestion predominate. In early ova, in which the apposition of

the two portions of the placenta is incomplete, the vascular condition of each admits of being more easily distinguished. We believe it may be generally stated that maternal congestion is more frequent in early ova; and that fœtal congestion, or the mixed form, is more frequent in older ova. Fœtal congestion, in its simplest form, may be observed in cases of delivery at the full term, in which the child is born alive, the cord having been tied on the placental as well as on the fœtal side of the point of division by the scissors. In such a case, the vessels of the cords are seen to be greatly distended, presenting the appearance of varicose enlargements. Tracing the vessels back, a similar appearance is seen on the fœtal surface of the placenta. The whole mass of the organ is firm, rounded, of a dark purple colour, and gorged with blood. The vessels in the villi, if examined under the microscope, are seen to be crammed with blood-corpuscles, and enlarged in their dimensions from distension.

The illustration given by Professor Simpson marks a second and more advanced degree of congestion. He refers to the condition of the placenta in cases in which the heart of the child has happened to be long impacted in the passages of the pelvis.

"The appearance which the placenta exhibits on its being expelled after such cases, and more particularly if the impaction has been so great as to prove fatal to the child, are well known to every practical accoucheur. The external surface of the organ is of a more or less deep violet, and sometimes almost livid colour; its internal structure, when torn or divided by the scalpel, presents a deep purple hue; its vessels are everywhere distended with dark-coloured blood; the organ appears enlarged, and its substance feels heavier and more solid than natural."

Rokitansky gives a precisely similar description of the appearance of congestion of the placenta. We witness the counterpart of congestion of the placenta in the intensely livid hue and swelling of the face of the child, arising from stagnation of the blood from long-continued pressure. The cases cited are examples of *fœtal* or *chorial* congestion. If we bring to our aid the physiological homology of the placenta, and the air-breathing lung, we shall have no difficulty in understanding how the placenta may be exposed to congestion, inflammation, and effusions from analogous causes to those which induce similar lesions in the lung. In the adult, death by asphyxia is revealed by inspection of the lungs. In the fœtus, death by asphyxia is read in the placenta. The true fœtal trachea is constituted by the utero-placental arteries, which convey to the cavernous structure of the placenta the oxygenated blood of the mother. If this flow be intercepted, the fœtus dies of suffocation. If blood not duly oxygenated, or blood impregnated with some noxious ingredient, is supplied, again, the fœtus dies of asphyxia or of poison, just as the adult would perish if made to inhale carbonic acid gas. This is not a matter of theory, but of observation. We are not aware that in the entire range of medical literature an experimental observation more apposite or more interesting can be found than the following. A case occurred, in which we deemed it necessary to bring on premature labour at the seventh month. When the labour had made some progress, the cord fell through the os uteri into the vagina; by holding this lightly in the fingers, we were enabled to feel the pulse of the unborn child. When the uterus was quiescent, the pulsations of the cord were 80 in the minute, and strong.

The torpid uterus was roused to action by galvanism. During every contraction so induced, the pulsations became first intermitting, feeble, and then stopped. Had not the galvanic stimulus been withdrawn, the child must have died of asphyxia. Withdrawn in time, the uterus relaxed, blood flowed again into the placenta, the fœtal circulation was again set in motion, and the pulsations returned. Presently, uterine contractions came on spontaneously. The same phenomena were observed. Nothing could prove more clearly that uterine contraction, in compressing the utero-placental vessels, acted in precisely the same manner as a ligature upon the windpipe. In another case of premature delivery, brought about by attachment of the placenta over the os uteri, the following phenomena were accurately noted. In the absence of uterine contraction, the fœtal heart was heard by the stethoscope beating 90 times in the minute. During uterine contraction, the pulsations fell to 60. After the expulsion of the child, which was scarcely sufficiently mature to be viable, the uterus contracted firmly, but the cord was not severed at once. The firm contraction of the uterus, we believe, arrested the placental circulation. The child's life depended upon breathing air. It gasped feebly; the heart beat 90 in the minute. The gasp at an end: the pulse fell to 60. Respiration excited artificially: pulse rose immediately to 90; and dropped again to 60 as respiration stopped. And so, for a considerable time, the pulse ebbed and flowed, as respiration ceased or was renewed; and this even after the cord was severed. It will be observed that the effect of respiration by the lungs upon the fœtal pulse was precisely the same as the effect of placental respiration had been. It matters not whether the oxygenating medium be brought into contact with the fœtal blood through the aërial trachea of the born infant, or through the sanguiferous trachea of the utero-placental system.

Whilst these precise observations, which only confirm the general testimony of physiology, prove the homologous functions of the placenta and the lungs, observations in morbid anatomy tell the same thing. Bayard, Casper, Ritgen, Cruveilhier, Litzmann, Krahmer, Hecker, and others have described the post-mortem appearances in numerous cases of fœtal asphyxia, occurring from a variety of causes. The constant appearances were punctiform ecchymoses, scattered over the pleuræ and pericardium, resembling in character those found in asphyxia in the adult. It may seem unnecessary to dwell upon this analogy, or rather identity, of function. But it is not so. The entire physiological importance of the placenta has been very recently called in question; and if the physiology is null, the pathology is null also. Dr. Druitt has contended that—"The placenta being, *par excellence*, a temporary organ, might fairly be expected to be prone to degenerate towards the close of its term of office;" and that—"Incipient degeneration is a normal condition of the placenta at the end of pregnancy." Again, in commenting upon the appearances exhibited in the organs of a fœtus which had died in utero, a day or two before the expiration of the full term of gestation, Dr. Druitt pointed out that—

"The mother was in perfect health, and had undergone no illness which could account for the death of the fœtus. The placenta exhibited no more than the *usual amount of alteration*. The abdomen, pericardium, both pleuræ, and the pla-

tinous tissue of the cord, was filled with bloody serum; and *the surface of the heart and lungs was covered with small ecchymoses.*"

He alluded to the obscurity of the causes which produced such a fatal alteration in the character of the blood in this case, and to the necessity of examining the bodies of fœtuses. No doubt, the cause of death in this case may seem obscure; but Dr. Druitt, entertaining a very mean opinion of the placenta, or of its use towards the end of gestation, does not hesitate to place that cause in the fœtus. But what organ was there in the fœtus, the functions of which were of equal importance with those of the placenta? And the placenta was diseased. What were the morbid appearances observed? Ecchymoses on the serous membrane of the heart and lungs—that is, the constant appearances found in death from asphyxia, in death from interception of the relations between the fœtus and the placenta. But it is said that the alteration in the placenta was of "no more than the usual amount."* What is the usual amount? Is it true that, in proportion as the fœtus grows in size, as its assimilative and eliminative functions increase in activity and extent, that the organ which is the chief agent in all these processes becomes worn out, and less and less useful? If the placenta become partially disabled towards the end of gestation, what organ takes up its functions?—for those functions must be performed. And if so, the placenta, in all its integrity, can no more be spared, even up to the very eve of being superseded by the lungs, than can, from that moment, the lungs themselves. In discussing the history of degeneration of the placenta, we shall have occasion to show that the facts upon which Dr. Druitt rests his singular physiological heresy have a very different significance from that which he assigns to them. We proceed with our subject.

If the cord be obstructed, the fœtus will equally perish. The obstruction of the cord is the same thing as tying the pulmonary arteries and veins in the adult. Asphyxia results; and the consequence of asphyxia is again seen in the placenta. This consequence is congestion; and the congestion will be both maternal and fœtal. But asphyxia may happen by other modes than those we have referred to above. In the same way as pleuritic effusions may compress the lungs, as exudations, inflammatory and of other natures, may render a large portion of the lungs unfit for their function, as degeneration or other alteration of the elementary tissues of the lungs may equally render them unfit for their function, so may pressure, inflammatory and other exudations, alteration or degeneration of tissue, arrest the function of the placenta: the asphyxia resulting may be rapid, or gradual and slow.

Congestion of the fœtal vessels, carried to a high degree, may lead to sanguineous effusion. Congestion with certain diseased modifications of the fœtal blood, may lead to dropsical effusions, or fibrinous effusions. We have not observed any clear examples of simple sanguineous extravasation from the fœtal vessels. Dr. Radford has described cases of hæmorrhage arising from the accidental rupture of these vessels. But this is a different lesion from that we are considering. That state of the placenta which has been frequently called "apoplexy," and which consists in a circumscribed effusion of blood in the placental parenchyma, is essentially a disease of the decidual or maternal side of the organ: we, therefore, for the present, postpone the considera-

tion of it. That dropsical effusion may sometimes arise from the fœtal vessels, we have no doubt; and we shall have occasion to describe examples in point. Whether those fibrinous masses, of variable shape and size, so often seen through the fœtal membranes on the fœtal surface, or surrounding the margin of the placenta, more frequently owe their origin to the fœtal or the maternal circulation, we are, at present, unable to decide. That they are not always, or even generally, the reliquiæ of blood-clots, as many believe them to be, we think, is certain. The examination of the nature of these deposits will also be more conveniently taken up further on It may be confidently assumed that wherever bloodvessels and blood are found, there inflammation may occur. We may therefore conclude, that inflammation may arise in the fœtal side of the placenta. That inflammation may occur in the peritoneum, and other organs of the fœtus, is a matter of constant observation. In so far as inflammation is dependent upon an altered condition of the blood, it is, then, clear that, since the blood in the fœtal vessels may be altered in a similar manner to that in the abdominal viscera of the fœtus, so inflammation may, in like manner, arise in the fœtal placenta. We are not, however, at present, able to verify this *à priori* conclusion, reasonable as it is, by positive observation. A marked case of recent inflammation, exhibiting the ordinary traces of exudation in the villi of the placenta, we have not yet seen. To what extent the fibrinous deposits, so frequently seen on the fœtal surface of the organ, or atrophy, or fatty degeneration of the villi and bloodvessels, are to be admitted as evidences, as results, of bygone inflammation, is a point that admits of considerable discussion. Those pathologists who deny the possibility of inflammation in the placenta, and those who look upon inflammation as the principal disease to which the placenta is liable, tracing all other lesions to that as their source, of course see no difficulty in the matter. In their minds, the whole question is prejudged. But admitting that it may be occasionally true, that the morbid appearances referred to are the effects of antecedent inflammatory action, we believe that, in the majority of cases, these appearances are due to causes not of an inflammatory nature.

The following case related by Dance, is an interesting example of inflammation of the membranes:

"Obs. xxi. A workwoman, aged 20, aborted at the fourth month of her first pregnancy, having previously exhibited febrile symptoms. The fœtus was of the fourth month, the skin red, and showed no signs of life. The fœtal surface of the placenta presented a singular yellowish-white colour, similar to that of the false membranes of the pleura. Scraping removed nothing. This membrane being carefully removed, we found between it and the chorion a plastic layer of false membranes, thin and soft; and even a little true pus. This purulent layer was spread over the entire inner surface of the placenta, but unequally. The chorion was twice or even three times the normal thickness; it was hard, as if scirrhous, and quite opaque; the thickest portions corresponded with those parts where the false membranes were the most abundant."

A case related by Ollivier d'Angers, seems also to be a clear example of inflammation of the fœtal membranes.

"Mad. ——, 18 years of age, had reached the fourth month of her first pregnancy without accident, when she fell ill, had constipation, a red discharge, pains in the loins, pain on pressure over the abdomen, and slight fever. On the fifth day there was enormous distension of the abdomen. By rest, low diet, &c., these symptoms disappeared. She was delivered naturally of a living child. An hour

before the descent of the head, a tumour of the size of the fist suddenly came down. It was of a dull-white colour, and was found to be formed by the membranes, which had altogether the appearance and thickness of parchment which has been soaked some time in water. This bag, filled with liquor amnii, remained thus at the valve until it was burst by the descent of the head. The membranes, throughout one-third part, were considerably thickened, whitish, opaque, and villous on their internal surface. The thickened portion was traversed by very fine vessels in the neighbourhood of the placenta"

A thickening and opacity of the membranes, such as Danée and Ollivier have described, are not uncommonly observed. We have met with one case in which the membranes were so thick and strong, that labour was altogether prevented from this cause. The liquor amnii could not be discharged until an opening was made in the presenting pouch by the scissors. No amount of force that the fingers and nails could exert was of any avail. Examples have also been seen of extensive vascularity, conjoined with thickening and opacity; these evidences of inflammation occupying a part or even the whole membranes.

In 1809, M. Mercier published a most interesting paper on dropsy of the amnion, which he attributed to inflammation of that membrane. He relates three cases. The first is that of a woman five months pregnant, who, after being fatigued and overheated, drank a quantity of cold water, and was, in consequence, seized with pains in the pubes and loins, cold shivering, nausea, anxiety, and cough. The pain in the lower part of the abdomen increased, and the hypogastrium became tense and swollen. On the 16th day, the abdomen became greatly enlarged, labour-pains came on, and ten pints of liquor amnii were discharged, and afterwards two fœtuses, which scarcely showed any signs of life, were expelled. The fœtal surface of the amnion was partially coated with false membranes, and the amnion itself covered with bloodvessels of a rose-red colour. In the second case, the infliction of an injury during pregnancy was soon followed by vomiting, and lancinating pains in the hypogastric region and pyrexia. On the 10th day after the accident, the pains having been relieved by bleeding, the abdomen began to acquire an unusual size. On the 43rd day, the abdomen became enormously swollen, and respiration was laborious. The membranes were soon after punctured with a long needle, and as the water flowed, the swelling gradually subsided; labour-pains came on on the following day, and two dead children were expelled. In this case, about a quarter of the fœtal surface of the amnion was inflamed, being of a deep-red colour, and double the natural thickness. The history and morbid appearances of the third resemble those of the two preceding cases.

In commenting upon these cases, Dr. Lee remarks, that "in very few of the cases he has seen, has the formation of an excessive quantity of liquor amnii been accompanied with inflammatory and dropsical symptoms in the mother, and in none did the amnion exhibit those morbid appearances produced by inflammation, which M. Mercier has described." In some cases of excess of liquor amnii which we have observed, we have also verified the absence of inflammation of the amnion. Still M. Mercier's cases are precise in their details. We may admit that inflammation is an occasional cause of dropsy of the amnion; but it is equally certain that excess of liquor amnii may arise from a variety of other causes. We are not of opinion that thickening of the coherent amnion and chorion always

depends upon inflammation; or even that the presence of a layer of fibrin upon or between these membranes is always owing to that cause. We shall hereafter have occasion to discuss the nature and origin of fibrinous deposits.

Of all the diseases to which the placenta is liable, that which is most undoubtedly local and peculiar to the chorion, is the hydatidiform degeneration. There is no other disease, the general appearances of which are so familiar to the obstetric practitioner. When this disease has attained an advanced stage, there is no overlooking it, and no mistaking its nature. In its incipient stages, in some cases of very early abortion, we have, however, known it escape detection, until the chorion was examined with a lens or the microscope. Almost the entire interest, in an anatomical point of view, of this remarkable affection, is centered in the question of its origin. From the moment that this morbid conversion of the chorion begins, the safety of the embryo is assailed. The instances are rare—if, indeed, any such exist—of the birth of a living child in connexion with a hydatiginous placenta. In the greater number of instances in which the placenta has undergone this change, the embryo is found of a size so minute as to be strikingly disproportionate to the bulk of the placenta; and not seldom not even a trace of an embryo can be discovered. The morbid developmental power soon conquers that of the embryo, and usurps its place. The embryo cut off, the diseased placenta continues to grow for itself.

It is unnecessary to refute in this place, and at the present time, the obsolete doctrine out of which arose the name of this disease, which recognized in the characteristic vesicular growths true hydatic parasites. Since the days of Percy, we are not aware that any observer has seen any evidence of independent animal life in the vesicles of the hydatidiform placenta. No one, we believe, has seen anything to confirm the following statement:*—

"J'en ramassai un plein grand verre (of the vesicles), que je portai aussitôt à M. Hermann pour le rendre témoin des mouvements que j'avais aperçus dans le premières; mais la plupart étaient mortes, et les autres si languissantes, qu'elles ne nous donnaient que de tres-faibles signes de vie."

In passing, we would observe, that Percy, being firmly convinced of the independent animal nature of hydatids, chivalrously defended the chastity of two women who were delivered of hydatid moles!

That method of minute structural analysis which has in recent times cleared up so many histological questions, has demonstrated that this disease, like almost every other that we shall have to consider, consists not in the implantation and growth of elements foreign to the placenta,—that is, in a new growth,—but in a perverted development or involution of the proper component structures of the organ. Indeed, we may state it as a general rule, that heteromorphous formations are far more rare in the placenta than are alterations of normal tissues. These last embrace by far the most important part of the pathology of the placenta.

Sœmmering, Lobstein, Andral, Cruveilhier, and Moreau believed that the cysts arose from an obstruction and partial expansion of the bloodvessels of the placenta. More minute observations, however, prove that the view entertained by these illustrious pathologists, as to the genesis of the cysts,

* Mémoire sur le Part Hydatique: Journ. de Méd. Chir. et Pharm., 1811.

is not correct. Rokitansky agrees with all those who have in recent times carefully investigated the subject, in attributing the origin of the cysts to a degeneration of the villi of the chorion.

The hydatidiform degeneration of the placenta presents as striking an example as any that can be adduced of the origin of abnormal or pathological structures, from the perverted or aberrant developmental tendency of normal or physiological elements. In order to understand rightly the histology of the hydatidiform chorion it is, therefore, necessary to observe the mode of growth and extension of the healthy villi. If the shaggy chorion of early ova, of that period when the villi are in a state of active growth in order to form a placenta, be examined under a moderate magnifying power, innumerable bud-like processes of varying shape will be seen springing from the extremities and sides of the perfect villi. These bud-like processes are pyriform, clavate, or fusiform; they vary in length and form, from the mere buds sessile upon the parent-villus to prolongations bearing more and more distinctly the ordinary characters of villi. There can, in short, be little doubt, that the growth and increase of the number of the villi take place through this process of gemmation and ramification—a process that may be most accurately compared to the growth of the roots of a tree. These budding villi are thus referred to by M. Désormeaux:*

"Velpeau has seen in ova of one month, or six weeks, the extremities of an infinite number of lateral ramuscles to present a sudden, rounded, or ovoid dilatation, having the appearance of a vesicula; these vesicles exist also in great number on the continuity of these ramuscles, so that these vascular branches present the appearance of a bunch of currants, or better still, of one of those bunches of vesicles, which constitute the hydatic mole."

We have not ourselves observed a specimen of healthy chorion in which the dilated processes existed in the exaggerated form described by Désormeaux. We are disposed to infer that the villi which formed the subject of his observations, were in reality morbid, and in process of hydatidiform degeneration. But if we subtract somewhat from his description; if we figure to ourselves the dilated bodies less numerous and less uniformly vesicular, then we fall easily within the physiological limits, and possess an image of a condition which is often observed in the healthy chorion. Seiler, and Vrolik, who reproduces Seiler's plate, appear to regard these vesicular processes as of normal occurrence, but as not persisting, unless under morbid conditions. Vrolik thus expresses himself:—"Flocci secundum Seiler a quintâ vel sextâ graviditatis hebdomade, vesiculis terminati, *quæ dein evanescunt,* sed nonnunquam abnormi ratione permanent, et tunc isti spuriæ graviditatis speciei ansam dant, quam molam botryoidam vel hydaticam dicunt." The drawing thus referred to is rather rudely executed; but the chorion, as represented, appears to be thickened, opaque, and partially detached—appearances strongly indicative of fatty degeneration. This change may have been consecutive upon the vesicular change of the villi. It seems to us more probable that the vesicular processes in this case, also, were the commencement of a true hydatidiform degeneration, than that they were simply healthy budding villi. With regard to the remark that these vesicles disappear (*evanescunt*) at a later period, we would observe that they only disappear by changing

* Dict. de Méd., Art., Œuf.

their form; they are lost as vesicles, because they become developed into villi. That the development of new villi is really effected by the budding of new shoots from the extremities and sides of the old villi, is well figured by Schroeder Van der Kolk. The pyriform extension of young chorion-villi is also figured by Mettenheimer. Virchow also describes this as the regular process of formation. To this we may venture to add numerous observations of our own. We have found these bud-like processes far more constantly, and in greater numbers, in the early chorion and in young placentas, than in placentas of an advanced period of gestation. As we have already stated, they are by no means uniformly vesicular in shape, but pyriform, clavate, fusiform bodies, the narrow part connecting them, like a stalk, with the end or side of the villus; or sometimes the extremity of a villus appears enlarged, and divided into a number of lobes, no contraction of a part resembling a stalk being observed. In perhaps the earliest ovum we have had an opportunity of examining in the fresh state—one probably not four weeks old—the termination of *every* villus exhibited a simple or compound lobular appearance, or distinct projections; some villi had similar processes from their sides. At a later stage of growth, instead of the clavate or fusiform processes, there are seen cylinders of greater or less length, but still bearing a resemblance to the primitive shape in their somewhat dilated extremities: these are young villi. Often upon these, again, secondary processes or buds may be observed. Inasmuch as the rapid growth of the fœtus, towards the term of gestation, is ever calling for an increased expansion of placenta, it may be presumed that fresh villi are constantly growing. These buds are accordingly seen on the villi of placentas approaching maturity: but they are far less frequent than in early ova.

Now, it appears that under the influence of a perverted developmental force, these buds, instead of growing into villi, may dilate into true vesicles, or hydatidiform cysts. Such a perversion of growth necessarily involves the destruction of the placenta as a respiratory organ, and the consequent death of the embryo. Those who have examined a great number of ova of different epochs, and who have registered what they have seen, will have become familiar with various appearances which can neither be distinctly referred to healthy villi, nor to hydatidiform degeneration. They will have observed bodies attached to villi which, although evidently of the same origin as the ordinary budding villi, were yet so different in some of their characters as clearly to have failed as villi, and which, nevertheless, were not recognised as hydatidiform cysts. The following case is an example of this kind:—A woman, who thought herself ten weeks pregnant, aborted suddenly. The decidua and chorion were undoubtedly of an earlier date than ten weeks; it was judged from the appearance of these structures that the embryo had perished four or five weeks before the expulsion of the ovum; the decidua had retained some connexion with the uterus to the last. Viewed with the naked eye, the villi of the chorion were dull yellowish white; they did not float out freely; they presented numerous nodular enlargements on the sides and extremities. With an inch glass the enlargements were seen more clearly: they were then seen to be pear-shaped bodies, springing from the sides and ends of the villi by *very narrow* pedicles, altogether resembling hydatids which have shrunk from being kept a day or two. With a quarter-inch these enlargements

were found to contain granular fat, they were opaque, some quite dark. No embryo was discovered. These bodies did not possess the characters of healthy villi, and the great disproportion between their slender stalks and the diameter of the bodies themselves, as well as of the villi to which they were attached, precluded the idea that they could ever assume the function of villi. We will not, however, do more than express a conjecture that these bodies were in course of degeneration into hydatidiform cysts. In addition to the peculiar appearance of these bodies, the villi generally were affected with fatty degeneration. This change may possibly have been the first morbid process, and the cause of the destruction of the embryo, and of the abortion. It is one of those numerous cases which mark the imperfection of our knowledge of the pathology of the ovum. The appearances observed may, however, justify a suspicion that, if these hydatid-like bodies were not really budding villi in the incipient stage of cystic degeneration, they were examples of some other hitherto unrecognised form of degeneration.

But, quitting this doubtfully intermediate state, let us pass on to the clearer forms of cystic degeneration of the chorion. A very clear account of this affection has been given by Mettenheimer.* This very careful observer objects to the comparison made by Cruveilhier of hydatid placenta to a bunch of grapes. He thus draws the distinction:

"Whilst in the grape-bunch there is a central trunk and branches, the latter giving off twigs, each of which bears its berries, in the hydatid chorion the central trunk is wanting, the centre of the whole vegetation being a bladder (the chorion) on whose walls a new generation of cysts is formed, each one of which has in like manner the property of developing one or many daughter-cysts. Berry grows out of berry, and the stalks do not unite berries with principal stems, but berries with berries, and lastly with a central mother-cyst."

We may here observe, that Madame Boivin† gives a very correct drawing of the common aspect of the hydatid placenta, exhibiting the manner in which the cysts are connected with each other. In this drawing it is seen that the cysts are not attached individually by stalks to a central stem, but that, in many instances, series of cysts are connected together, as it were, by one string, one cyst supporting a second, and this in its turn a third, and so on.

Mr. Paget,‡ also basing his views upon the observations of Mettenheimer, regards the hydatidiform placenta as an example of cystic development. He says:

"A part, or even the whole of the chorion is covered with pellucid vesicles, with limpid contents, borne on long, slender, and often, branching pedicles. The cysts are usually oval or pyriform; their walls are clear, or have minute opaque dots; they may be either simple, or may bear others projecting from their walls. Dr. Mettenheimer has found that the minute dots besetting these cysts are 'villous processes,' exactly resembling those of the natural chorion, and growing from the walls of the cysts, either outwardly or into their cavities. In these villi he traced the development of cysts. In their natural state, they may be described as filiform or clavate projections, and composed of dimly-granular substance, in which are embedded minute nucleated cells. In this cystic disease, vesicular bodies may be seen scattered among the cells in the villi, which bodies are distinguished from the cells by their pellucidity, their larger size, and double contours; but from the

* Müller's Archiv, 1850. † Nouvelles Récherches sur la M[illegible] Vésiculaire.
‡ Lectures: Medical Gazette, June, 1851.

cells to these every gradation may be traced, so as to leave scarcely a doubt that the vesicles are derived from cells deviating from their normal characters. . . . . The whole process may, therefore, be probably thus described: Certain of the cells in the proper villi of the chorion, deviating from their cell-form, and increasing disproportionately in size, form cysts which remain connected by the gradually elongated and hypertrophied tissue of the villi. On the outer surface of the new-formed cysts, each of which would, as it were, repeat the chorion, and surpass its powers, a new vegetation of villi sprouts out of the same structure as the proper villi of the chorion. In these begins again a similar development of cysts; and so on, *ad infinitum*. Each cyst, as it enlarges, seems to lead to the wasting of the cells around it; and then, moving away from the villus in which it was formed, it draws out the base of the villus, which strengthens itself, and forms the pedicle on which the cyst remains suspended."

To this account we think it interesting to add Mettenheimer's appeal to the admirable researches of Dr. Hodgkin, for the purpose of drawing more especial attention to the relation subsisting between the cystic disease of the chorion and abnormal cystic development in other structures of the body:

"Hodgkin distinguishes two kinds of cystoid formation: in the first, the young cysts stand out upon the walls of the old ones, without any tendency to grow inwards or to become stalked; in the second, the secondary cysts grow inwards, as pear-shaped stalked growths, from the inner wall of the mother cyst, developing again new progeny; and so forth. In the bladder-moles, it appears, from my researches, that there is a third kind, which exhibits the reverse of the second of Hodgkin. In this kind, the secondary cysts grow in villus-form, from the outer surface of the cysts."

It is not consistent with the plan of this article to dwell much upon the bearings of the diseased conditions of the placenta upon obstetric practice; but the following observations will not be out of place:

Dr. F. H. Ramsbotham, in considering the subject of adherent placenta after delivery, says, "At other times again, but very rarely, the remaining portions of placenta (after removing part by hand, &c.) become the nucleus for hydatinous formations, and more rarely still, I believe, they are actually absorbed." The question thus raised is one of great interest. But we are not aware that any unequivocal case exists, upon which the inference that a portion of placenta left in the uterus, *the portion extracted at the time of labour being free from cystic degeneration*, has subsequently become the seat of cystic disease, can be firmly based. It does not seem very probable that this disease ever *begins* at a late period of gestation. It seems essentially an affection of the young chorion. Beginning thus early, one of the first and surest effects is to destroy the embryo while this is still very small in size. It is consequently found, in the great majority of instances, that when this disease exists no trace of an embryo can be found, or if found, that it is exceedingly minute. Is there a case recorded in which the placenta was found affected with hydatidiform degeneration, even partial, at the normal term of gestation, and concurrently with the birth of a fully-developed living child? We put our doubt in the interrogative form because, although we consider it at variance with the known course and history of the disease under consideration that such a case should occur, we are not willing to hazard an absolute denial. But, assuming it to be in the highest degree improbable that the chorion of the placenta approaching maturity should become the seat of cystic degeneration, how much more improbable is it that a *portion* of mature placenta, remaining attached to the uterus after delivery, should

then take on a new cystic life of this kind? What is the condition of a fragment of retained adherent placenta? The fœtal vessels with the villi of the chorion are torn across, and the fœtal portion of the placenta—that portion which is the seat of cystic disease—may fairly be said to be organically destroyed. The only portion of the placenta which preserves life is the maternal portion, the decidual or uterine element; and this is not susceptible of cystic degeneration. Whether the difficulties we have raised be valid or not, it must at any rate be admitted that the doctrine enunciated by Dr. Ramsbotham requires to be substantiated by facts rigorously analysed. The interest of the question at issue will appear from this: If it be true that a portion of retained mature placenta may, subsequently to delivery, undergo hydatidiform growth, then one conception will account both for the product of the delivery in the usual course at which a child was born, and a portion of placenta brought away, and for a subsequent delivery at a remote period from the first, at which a hydatid mole was expelled. If it be not true that a portion of placenta so retained can undergo this change, then it follows that, in any case in which a hydatid mole may be expelled after an interval more or less remote from an ordinary labour, *a fresh conception has occurred.* The medico-legal bearings of the case are then of the greatest importance.

One other observation we take the opportunity of adding. It is clear from the account we have given of the histology of the cystic disease of the placenta, that Mettenheimer and others regard it as a disease of the chorion, originating in an abnormal development of the cells observed in the villi. This is equally clear from the figures given by Mettenheimer. But these cells are regarded by Goodsir, and those who follow him, as decidual or maternal. It would thence follow, that the cystic disease of the placenta is not an affection of the fœtal element, but in reality one of maternal origin. We are not aware that any pathologist has advocated this view. It therefore appears to us that we possess, in the history of the cystic disease of the placenta, a confirmation of that view which regards the cells of the chorion as an integral part of the fœtal portion of the placenta.

Although it is in the highest degree probable that fatty degeneration of villi of the chorion may take place as a primitive condition, we defer the minute consideration of this affection until we take up the diseases of the placenta that may be traced to the mother or embryo. We will simply observe in this place that, according to our observations, a considerable amount of fatty degeneration of the chorion is commonly found concurrently with the hydatiginous degeneration. Case X., recorded in the reviewer's second paper in the 'Medico-Chirurgical Transactions,' is an instructive example of this kind. In most cases it is probably secondary upon the physiological destruction of the villi produced by the hydatiginous change. But independently of any hydatiginous affection, and independently of any morbid influence transmitted from the mother, or of failure of the developmental force in the fœtus, it seems not an unreasonable conjecture, that in like manner as by an abnormal excess of cystic developmental force the villi may pass into hydatiginous degeneration, so by an opposite defective developmental power the cellular chorion may pass into atrophy, and fatty degeneration may follow.

(To be continued.) *Robert Barnes.*

PART SECOND.

Bibliographical Record.

ART. I.—1. *A System of Instruction in Quantitative Chemical Analysis.* By Dr. C. R. FRESENIUS. Second Edition. Edited by J. LLOYD BULLOCK, F.C.S.—*London,* 1854.

2. *A Course of Practical Chemistry, arranged for the Use of Medical Students.* By WILLIAM ODLING, M.B., F.C.S., Professor of Practical Chemistry and of Natural History at Guy's Hospital.—*London,* 1854.

3. *Anleitung zur Qualitativen und Quantitativen Zoochemischen Analyse.* Von E. VON GORUP-BESANEZ. Zweite Auflage.—*Nurnberg,* 1854.

Guide to Qualitative and Quantitative Zoo-chemical Analysis.

4. *Anleitung zur Qualitativen und Quantitativen Analyse des Harns.* Von CARL NEUBAUER.—*Wiesbaden,* 1854.

Guide to the Qualitative and Quantitative Analysis of the Urine.

THESE four works on chemistry are adapted to special ends, and are therefore very different in scope and execution. The masterly work of Fresenius, it is scarcely necessary to say, treats of the whole subject of quantitative chemistry in the broadest way. It is not necessary to do more than announce the appearance of the second edition, which has evidently been most carefully prepared by Mr. Bullock.

The little work by Dr. Odling is one of more modest pretensions, though in its way of not less use. The preface informs us that it contains an outline of the course of practical chemistry as annually carried out in Guy's Hospital. Inorganic, toxicological, and animal chemistry are discussed in as many sections. We are rather surprised to find under the last head no notice taken of the new modes of analysing urine, by using known quantities of test solutions; and although Dr. Odling may consider that this did not enter into his plan, it would have been a useful addition.

Gorup-Besanez has attempted to do for medical what Fresenius has done for general chemistry. He has given a comprehensive account of the modes of determining the presence and the quantity of the various constituents of the body, and has had especial reference to the wants of the physician. He has also given a very excellent short account of the various animal principles.

The author of the last work, Dr. Neubauer, is assistant to Fresenius, and, under the direction of that able chemist, has compiled a very perfect code of instructions for determining, according to the most

approved and recent methods, the amount of the urinary constituents. We do not observe anything novel in the work. The method of Liebig is employed for urea, although all other accurate plans are also given. Liebig's plan for chloride of sodium, Breed's for phosphoric acid, &c., are all carefully detailed. Three plates of urinary sediments are added, from Funke's Atlas.

ART. II.—*Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M.D., Physician to the Hospital for Sick Children. Third Edition.—*London*, 1854.

WHEN two editions of a medical book on a special subject are exhausted in six years, it is a sure sign that that book has supplied a want, and has supplied it well. In the present edition of his well-known lectures, Dr. West has taken every pains to support the reputation they have already acquired. Many alterations are made in the text, and in foot-notes, and the bulk of the volume has been increased by sixty pages. A larger number of cases, too, has been brought to bear on every doubtful point, and the latest authors on every subject are carefully referred to. It is almost superfluous to say, that this work should be in the library of every one engaged in the treatment of the diseases of children.

ART. III.—*A Practical Treatise on the Diseases of the Eye.* By WILLIAM MACKENZIE, M.D. Fourth Edition.—*London*, 1854.

THE fourth edition of this standard work will no doubt be as fully appreciated as the three former editions. It is unnecessary to say a word in its praise, for the verdict has already been passed upon it by the most competent judges, and 'Mackenzie on the Eye' has justly obtained a reputation, which it is no figure of speech to call world-wide.

ART. IV.—*On the Topical Medication of the Larynx in certain Diseases of the Respiratory and Vocal Organs.* By EBEN. WATSON, M.D., Lecturer on the Institutes of Medicine in the Andersonian University, &c.—*London*, 1854.

TOPICAL medication to the interior of the larynx, by means of a sponge, was used by Sir Charles Bell, and afterwards by Trousseau and Belloc, but was first brought prominently forward by Dr. Horace Green, and we may add also, by the author of the work before us, for he was one of the first in this country to put Dr. Green's statements to the test, and he has written several valuable papers on the subject.

Much discussion has taken place as to whether Dr. Horace Green succeeded in really introducing the sponge into the larynx. Dr. Watson tries to prove that the entrance into the larynx can be readily accomplished, and he does most certainly show that bodies larger than the sponge have accidentally passed into the trachea, that the dimensions of the larynx in the dead body are such as to admit the sponge, and that

in the dead body the passage can be accomplished. But we do not see that he has been more fortunate than others in adducing absolute proof that the larynx has been entered during life; and, although we do not deny that this can be sometimes done, we are very certain that it is an operation of great difficulty, and that in the great majority of cases, the operator has simply passed the sponge down the pharynx and œsophagus. In his remarks on this point, Dr. Watson appears to us to have been really unwarrantably uncivil to Mr. Erichsen, who, in his late work on 'Surgery,' has expressed doubts of the possibility of the operation.

Whether or not the sponge enters the larynx, the fact remains that the application of caustic to the upper part of the throat, to the epiglottis, and it may be to the interior of the larynx, is of the greatest service in many troublesome and serious diseases. Dr. Watson prefers the nitrate of silver, but he has used also the hyposulphite of soda and silver, and has found it sometimes useful. The strength of the nitrate of silver varies from ten to sixty grains in an ounce of water.

In acute cases Dr. Watson's experience is thus summed up:—

"In acute laryngitis in which there is no false membrane, the local application of solution of caustic, varying in strength inversely in proportion to the intensity of the inflammation, may be employed with more or less speedy benefit.

"During the exudative stage of true croup, the stimulant application to the part affected is injurious, but when the disease begins to yield to antiphlogistic and other treatment, it may assist in the cure.

"There is reason to believe that, in many cases of croup, there is an active inflammatory stage prior to exudation, in which the disease may be checked by topical and other means, appropriate to such cases.

"Œdema glottidis, whether occurring as a primary disease, or as a complication of other morbid states, is always speedily relieved, and in some cases effectually cured, by the application of strong solutions of the nitrate of silver to the œdematous organ."

In chronic laryngitis and aphonia, the use of the caustic is illustrated by some very interesting cases, but the utility of the remedy is so thoroughly admitted in similar cases, that it would be useless to delay on the point.

Dr. Watson has employed the topical medication of the larynx in hooping cough. This was entirely a suggestion of his own, and it has been attended with the greatest success. He employs rather a weak solution, and touches at first only the pharyngeal membrane; then, after some few applications, he passes the sponge into the larynx. By this remedy alone he is able to give, by putting together M. Joubert's cases and his own, the following most favourable report on the plan:

Cured in two weeks	96 cases,	or	57·4 per cent.
„ in three to four weeks	61 „	or	36·5 „
Resisted the treatment	9 „	or	5·3 „
Died	1 „	or nearly 0·6	„
	167		

True spasmodic asthma has been treated in the same way, and with very great success. Although Dr. Watson believes that there is spasm

of the bronchi, as well as of the larynx, in this disease, he thinks that if the latter be overcome, the bronchial spasm soon yields.

In the so-called stomach and hysterical coughs, in laryngismus epiliptica (2 cases), and in the laryngeal complication of pulmonary phthisis, the topical medication has been by Dr. Watson, as by others, employed with excellent results.

We recommend this very practical and well-written work very cordially to our readers.

ART. V.—*A Treatise on Diseases of the Lungs, having especial reference to Consumption.* By ANTHONY WILLIAM CLARKE, M.D.—*London,* 1854.

THE author informs us in his preface, that "the sources of his information have been various," and "that nothing will be found in his book which will not be verified under the test of trial." He also states that "independent in circumstances, he commits his brain-child to the world, satisfied if the amiable reader approves."

It requires greater amiability than we possess to welcome this very feeble and decrepid "brain-child" in the way its parent seems to expect. We have read the greater part of the book carefully, but we can find little that can have sprung from Dr. Clarke's brain, except at second hand. Page after page is filled with the most familiar facts, and the "experience acquired in a practice of tolerable extent" has just sufficed to make a book, which any man with a few good treatises before him might have compiled. We are sorry thus to speak of a gentleman who is probably a worthy and respectable practitioner, but we cannot speak otherwise, and yet speak truly.

ART. VI. — *Principles of Comparative Physiology.* By WILLIAM B. CARPENTER, M.D., F.R.S., &c. Fourth Edition.—*London,* 1854.

To call Dr. Carpenter the most excellent of compilers, though no small praise, would be doing him but scant justice. He is much more than a compiler, for he can not only select and combine, but he can investigate and discover. To say that Dr. Carpenter collects the facts which other men have pointed out, is merely to say that he has accomplished one of the inevitable conditions of the proposed work; but to assert that he does no more than collect, is to overlook the numerous original observations he has made himself, and the novel results which he deduces from the observations of others.

We have already had occasion to point out that Dr. Carpenter possesses in a high degree two mental qualities seldom found combined—the industry which collects, and the genius which discovers. In scarcely any former work have these powers been so marked as in the present. In no work that we have ever seen have the phenomena of life been discussed so broadly and lucidly, and in none have so many facts been put more happily and fairly into their appropriate places.

The work is, in many respects, a new one; it consists of only half the former edition, for the author found he could not do justice to his subject without enlarging the volume to an excessive degree. He has preferred, therefore, making separate works of the Comparative and the General Physiology, and the volume containing the latter subject has yet to appear.

We should devote a longer space to this work than we can give it here, did we not feel that no review is really necessary, for it will be read by all physiologists, and, from its comprehensive nature, it is little adapted for an analytical summary. When the other volume appears, however, we shall take the opportunity of including this volume in a comprehensive review on the general position of physiological science.

The work is most beautifully and copiously illustrated, as usual in all Dr. Carpenter's productions.

ART. VII.—*Transactions of the Pathological Society.* Volume V. *London,* 1854.

IN announcing another volume of the 'Transactions of the Pathological Society,' we can scarcely do more than repeat the opinion so often expressed, of the extreme value and interest of the facts brought before the society. A marked feature of the work now consists in the reports, which are made on the exhibited specimens by gentlemen especially conversant with those means of investigation by which the nature of the morbid change can be best discovered. In this way some admirable original statements are called forth, and specimens are investigated much more minutely than could possibly be the case if the exhibitor alone examined them. This plan is about to be carried to a greater extent, and the society proposes to investigate, by combined observation, certain obscure or imperfectly known conditions in pathology. We shall look forward with the greatest interest to the results of this plan, and we are confident that in a little time they will not disappoint our expectations.

ART. VIII.—*A Disquisition on certain Parts and Properties of the Blood.* By DAVID TOD, M.R.C.S.—*London,* 1854.

MR. TOD'S work cannot be considered a common one. We are not certain that we ever before met so many novelties in so small a space. We had not got through twenty pages before we found that the blood-corpuscles, or hæmatozoa, as Mr. Tod calls them, are animalculæ, that they feed on and digest albumen, that they possess great generative powers, which vary, however, according to the time of year, so that we suppose they have their rutting season, and that they turn, or can turn rather, into spermatozoa. Further on (p. 197) we are told, that man was originally generated by a gradual change occurring in the hæmatozoa of a particular class of extinct animals, this change being the result of what botanists call a "sport;" and without entering into any long analysis, we may simply state that the entire book is filled with statements equally remarkable.

Our readers may suppose that this work is a "sport" of Mr. Tod's, but we assure them this is not the case. Our author is in sober earnest, and illustrates his opinions by some of the most singular experiments and processes of reasoning we have ever had the fortune to read. He has perfect faith in his own views, announces them with complete gravity, and although we are afraid he must abandon all hope of convincing any one, he may feel quite certain that he has produced a book which will rank among the curiosities of medical literature.

ART. IX.—*An Expository Lexicon of the Terms used in Medical and General Science.* By R. G. MAYNE, M.D. Parts II. & III.

PART III. of this excellent work takes us almost to the end of the letter H, and we anticipate that the author can have little difficulty in concluding his work in the six parts, as originally proposed. We shall examine the whole work critically when finished, and shall now only repeat the very favourable opinion which the perusal of the first part led us to form.

ART. X.—*Neurologische Untersuchungen.* Von Dr. RUDOLPH WAGNER. *Göttingen*, 1854.
Neurologic Researches. By Dr. WAGNER.

THIS volume consists of a series of papers presented by Dr. Wagner during the last seven years to the Royal Society at Göttingen, and published in their 'Transactions.' The subjects of the papers (28 in number) are very various, and embrace pretty nearly the whole subject of Neurology. Almost all the facts, however, have passed into the domain of established physiology, since the last paper (on Bernard's Discovery of the production of Diabetes by wounding the floor of the fourth Ventricle) is dated in 1852.

Still this volume will have a lasting interest for all who desire to have a connected account of the discoveries of its celebrated author.

ART. XI.—*Lectures on Polarized Light* By the late JONATHAN PEREIRA, M.D., F.R.S., F.L.S. Second Edition. Edited by the Rev. BADEN POWELL, M.A., V.P.R.S.—*London*, 1854.

THE great power of research, and the unusual facility in using the materials at command, which were Dr. Pereira's pre-eminent mental qualities, are manifested as much in this small volume as in the gigantic work on Materia Medica. The edition before us is much enlarged, not only by materials left by the lamented author, but also by interpolations by the editor. It is scarcely necessary to say that it is a clear yet comprehensive treatise on the subject, and is likely to remain for some time what it now is, a standard work on Polarized light.

ART. XII.—*Summary of New Publications.*

WE have already noticed the majority of the works published up to the end of November which have reached us, and a very brief summary will suffice for the rest.

The thirty-seventh volume of the 'Medico-Chirurgical Transactions' is rather thinner than usual, and although there are some papers of great interest in it, it does not, we think, equal its immediate predecessors.

In *Medicine*, the concluding part of Wunderlich's extensive work* has appeared. Dr. Tanner's compendium of 'The Practice of Medicine' has reached a second edition. Although the descriptions of the different diseases are extremely short, the little work may be useful to a busy practitioner.

Dr. Davies has published a second edition of his 'Lectures on the Physical Diagnosis of the Diseases of the Lungs and Heart.' The most important addition appears to be an investigation into the frequency of venous murmurs among strong, healthy men, such as the Foot Guards. In fifty picked men of the Coldstream Guards, between the ages of 21 and 27, Dr. Davies found a venous murmur in the neck, either on one or both sides, in no less than forty-four. He confirms, then, the statements of Wintrich. The remainder of the volume has been very carefully revised.

Dr. Ballard has written a very elaborate work on a single symptom—viz., 'Pain after Food.' We shall review it in our next number.

The causes and extent of the epidemic of yellow fever which decimated New Orleans in 1853 have been recorded by Dr. Fenner, a well-known writer on this subject. The sanitary condition of New Orleans is described with care; and without denying the occasional contagion of yellow fever, Dr. Fenner is a warm advocate of its propagation in other ways, through the medium of a vitiated atmosphere.

Another American work, relating to a different department of pathology, will also be found to be interesting to a special class. We refer to Dr. Pliny Earle's treatise on 'Bloodletting in Insanity.' A great number of authorities are examined for and against this practice, and the result is, that Dr. Earle believes that insanity *per se* is a contra-indication for bloodletting, but that there may be conditions of plethora, or tendency to apoplexy, which demand it.

A second edition of 'What to Observe at the Bedside and after Death, in Medical Cases' has been issued. We have already reviewed the first edition, and shall therefore content ourselves with this simple announcement.

Almost simultaneously, two manuals of 'Pathological Anatomy' have been published: one in this country, by Drs. Handfield Jones and Sieveking; and one in Germany, by Dr. Förster,† of Göttingen. Both are works of a high order; and we anticipate for the English treatise a wide circulation in this country.

After the review on Electricity was in type, we received Duchenne's long-promised work. It is a bulky volume of 926 pages; but its pith has been already anticipated in the review published in this number. If necessary, however, we shall refer to it hereafter.

In *Surgery*, a large work on 'White Tumours of the Articulations'‡ will be noticed in the next review on Diseases of the Joints.

Mr. Skey has written a short pamphlet on 'The Relative Merit of Lithotomy and Lithotrity,' in which a very decided preference is given to

* Handbuch der Pathologie und Therapie.
† Handbuch der speciellen Pathol Anat. Göttingen, 1854.
‡ Traité des Tumeurs Blanches des Articulations, par le Dr. J. Crocq. Bruxelles, 1854.

the latter. The subject is too important for us to discuss here, but we shall examine it thoroughly in an early number.

A pamphlet of interest has been written by Mr. Ward—'On Strangulated Hernia;' and is based on the experience of three years at the London Hospital. During this time, 242 cases of hernia were admitted, and 69 were operated on—viz., 43 femoral, 22 inguinal, and 4 umbilical; of this number, 21 died. In the cases of femoral hernia (39 of which were in women), the sac was not opened (Luke's operation being usually employed) in 29; the average period of strangulation was slightly over thirty-four hours; 4 cases died, or 13·85 per cent. In 13 cases, the sac was opened; 6 died, or 46·1 per cent.; the average time of strangulation being fifty-eight hours. This would appear to show that one operation is vastly superior to the other; but Mr. Ward points out the difficulty of comparing the two classes of cases; and although he evidently inclines to Mr. Luke's operation, hesitates to draw the strong inference which might at first sight be drawn from the above facts. Many other interesting points are discussed in the pamphlet, which, we have little doubt, most of our readers will peruse for themselves.

We have room only to enumerate the numerous other works on various subjects which have reached us, and to some of which we must return. Mr. Child has published three lectures on 'Injuries incidental to Warfare,' but has neither done justice to his subject, nor to himself. A second edition of Mr. Hunt's pamphlet on 'Syphilitic Eruptions' would have been more useful had the cases been given in greater detail. 'A Discourse on Medical Botany,' by Earl Stanhope, is an interesting and well-written theme on the benefits to be derived from an assiduous attention to the medicinal virtues of plants; and a paper by Mr. Lizars, on 'Tobacco,' is a violent tirade against the use of the "grateful weed," and, like most tirades, is only one-sided. The 'Watering Places of England,' by Mr. Lee, has passed into a third edition; a concise account is given of all the places of resort in this country. In a pamphlet entitled 'Harrogate and its Resources,' some very valuable analyses, by Professor Hoffmann, are given at length. The 'Book of Prescriptions,' by Mr. Beasley, is one of real value to practical men, as it contains numerous formulæ gathered from all sources, and embracing almost all the possible combinations in prescribing. The fifth part of the 'Micrographic Dictionary' brings us down to the letter C; there is no falling off from the excellence of the previous parts.

We reserve for special review some other works:—viz., Mr. Henry Lee's 'Pathological and Surgical Observations;' Mr. Swan's work on the 'Brain and Mind;' Dr. Bucknill's prize treatise on 'Insanity;' and two German inaugural dissertations of considerable merit—viz., one on the 'Adenoid Tumour of the Female Breast,'* and the other on the 'Tumours which occur in the Course of Nerves.'†

* Das Adenoid der Weiblichen Brust. Von A. Weber. Giessen, 1854.

† Ein Beitrag zur Pathol. Anatomie der Geschwülste in Verlaufe der Nerven. Von F. Kupferberg.

PART THIRD.

Original Communications.

Art. I.

The Blood—its Chemistry, Physiology, and Pathology. By Thomas Williams, M.D. Lond., Licentiate of the Royal College of Physicians; formerly Demonstrator on Structural Anatomy at Guy's Hospital, and now of Swansea.

(*Continued from No.* 25, *p.* 207.)

Human physiology is only a solitary planet in the orrery of organic science. Detached from the mutually dependent and reciprocally illustrative units of the system, it becomes incomprehensible. Viewed as the *summa philosophia*, the pinnacle of organization, it is the *terminus* on which converge a thousand paths of elucidative research. Every inferior animal successfully dissectionized, uplifts some veil, dissipates some mystery, unlocks the long-chained cavern of some new springs of revivified thought, in its utilized reference to the science of the human organism. Who does not hear, in this methodological principle, the voice of admonition? Does it not point the way? Does it not exhort the child to walk first? To the philosopher does it not speak, begin where nature is intelligible? Examine the whole animal kingdom as an indivisible multiune machinery; regard the consecutive members of the series as stages in the growth of one animal; contemplate it first in its living totality. Does it not bear the impress of a dynamic, as of a material unity? Does it not look like one body obeying the dicta of one life-power of commensurate stupendousness? Yes. But where is the painter so gifted as, even in faint and dubious outline, to reduce to visible form a conception so splendid, a picture so grand? Was it not traced by the master-hand of the great physiologist of antiquity? In his 'Περὶ Ζώων Ἱστορίας,' did not Aristotle forefigure the true principles of animal classification? Did he not pile Ossa and Olympus on Pelion, in order to scale the battlements of his mythic heaven? Did he not superimpose the zoophyte on the plant, the star-fish on the zoophyte, the fish on the mollusc, and the mammal on all, in order to make a man? Inanimate matter grows into the vegetable form, and the latter rises to the excellence of an animal: *that* is the philosophy of Aristotle. It is the primæval germ of all later conceptions as to the "unity" of organization. It is, indubitably, prophetic of the tendency of modern science. This very sentiment, in its unavowed essentiality, haunted the secret aspirations of Cuvier. Though repudiating the possibility of linking the countless members of the organized world into an ascensive chain of linearly or circularly successive sequences, he unconsciously superscribed his monumental 'Regne Animal' with the presiding

conviction of his mind—his secret *regulus philosophandi*, "*distribué d'apres son organisation!*" Goëthe and Oken, Geoffroie St. Hilaire, Lamarck, and an army of less distinguished thinkers, have since preached upon the same cosmogonic text, upon the doctrine so monosyllabically expounded by Ben Jonson:

> "Nature doth first beget the imperfect, then
> Proceeds she to the perfect."

Plato and Socrates shaped into immortal form an identical idea. The scheme of creation, the archetypal idea, existed in the mind of the Creator anteriorly to the first act of zoo-genesis: such was *their* philosophy. They pretended, as the modern world-creators have fondly pretended, to have discovered the peep-window commanding a perfect view of the wondrous panorama. The author of the 'Vestiges' professes to have seen the workman at his cyclopean work; to have caught him handling the primitive, plastic clay, the formless *nebulæ;* then to have tracked him in his artful evolutions, until at length his mystic labour issued in the apparition of the *summa*, the Caucasian type-man! These visions are profane, because they falsify the true *method* of nature. Such is the indignant criticism of the clear-minded Professor Sedgwick. It is intolerant to argue in such a manner. What! Is it irreverent to catechise creation as to whence it came, or whither it goeth? Is it profanity to study the *method* of things? Then it is impious to observe the signs of things; to gaze, though with awe-stricken humility, upon the soulless phenomena of this mighty world! All hostile eloquence is vain. The human mind ever has yearned, is yearning, and will for ever intensely yearn, after a perfect knowledge of the *reason* and the *meaning* of visible things. To reveal the order of causation is the sweetest fruition of the finite intellect. The passion will never cease to burn.

Modern science has shaped this great cosmogonic controversy into three grand parallel arguments: 1. The geologic; 2. The embryologic; and, 3. The zoologic. Many individual links of these chains respectively remain to be forged, by discoveries yet unaccomplished; but every living philosopher must really feel that there runs the golden thread of *unity* severally through these series. The conviction cannot be parried away: it reappears, like an all-pervading divinity, in every insignificant event. It must be true. The appearance of living beings in geologic time is the most vulnerable point in this beautiful tripartite argument. It is vulnerable only when contemplated from the insecure eminence of presumptuous theories. Let it be granted that the vertebrate *did* appear on the theatre of objective realities before the invertebrated animal: what does it disprove? Most certainly not the *serial* method of creation—not the consistent homogeneity of the principles of organization. Though only man may at this moment flourish on the scene of this sublunary world, can it be argued that the Diatomian was not *intended* to be lowest of all animal forms—that is, was not the normal continuation downwards of that organized series whose uppermost members appeared first on the theatre of created beings? Though the highest fish may have preceded the lowest in the palæontological scale, it only proves that the highest was not formed out of the lowest; it does not militate against the doctrine of *linearity* in the march of organization. If the alleged facts—viz., that

the geologic series are contradictory of the idea of progressive development—be true events in nature, they really only demolish the hypothesis of the transmutation of species. If the archetypal scheme was *mentally* perfect from the beginning, the arrangement was not disturbed, though the fish may have anteceded the cephalopod. Let it be conceived that the physical conditions proper for birds occurred *before* those consistent with fish-life, it does not annihilate the Platonic idea that, in the unwritten plan of cosmos, the fish was normally predestined to stand below the bird. It only erects into loftier eminence the orthodox argument of *volitional* creation; but it lends support in essence to the *principium* of spontaneous generation. A species is evolved only when a certain definite assemblage of material conditions is realized. Inorganic forces are transmuted into the "vital principles," in accordance with the modern doctrine of "the convertibility of forces." Such "conditions" conspire to produce a result. The conditions are the *final* secondary causes. The word "spontaneous" is an inapposite expression. It invests conditions with creative, volitional spontaneity. The inference is a vitiated, corrupt *lemma*, unfairly planted on splendid premises. In all nature, living and unliving, do not one class of events conditionate other events? Do not the latter rest on the substratum of the former? "Spontaneous generation!" It most certainly never was intended that the mind should *terminate* in the word "spontaneous." If it be signified, though only by conventional implication, that all "conditions," all "final and secondary causes," are swayed, directed—aye, created, by a fore-going, fore-being primal Power, it does not contravene the *principia* of "spontaneous generation," it does not destroy the *idea* that the Creator should act, not by partial but by general laws, should *create*, that is, through the instrumentality of directive "conditions."

The plan of the animal kingdom is unquestionably founded on the idea of consecutive progression, not *necessarily progression upwards.* The march may be *downwards,* in the direction of simpler organisms; or, laterally, into dependent, but divergent lines. The lowest members of this progression, *when present,* are the simplest, the highest, the most complex. It is palpably true of the vegetable kingdom. Though not capable of proof in all its details, the *principle* of ascensive seriality is true, also, in palæontology. The argument of serial evolution is, emphatically, cumulative. Every discovery adds to its cogency. The word *comparative* anatomy originated with Condorcet, the French metaphysician. He saw in it deep, unreached meaning. In persuasiveness, it is every day increasing. The "serial homolotypes" of Sir Charles Bell and Professor Owen are the riper fruit of the same far-extending conception; without it, neither comparative anatomy, nor comparative histology, nor the vital study of comparative physiology, can have any pretensions to a secure rank in the ennobled hierarchy of the sciences. But let all this be most earnestly explained. The alleged progression of grades in the zoological, or geologic, or embryonic scale, involves one question; continuity of type in organs and their histological elements totally another. The latter thought is new, the former is as ancient as Plato, and Aristotle, and Socrates. The fish bears not the faintest semblance to the likeness of the bird. The two animals are irreconcilably diverse; but in ultimate constructional

elements, the liver of the former is identical with that of the latter! Postpone the details of the argument. Throughout the extant kingdom of animals, there prevails a recondite conformity of *structural principles*, which theorists cannot, at this age of the science of observation, dare to dispute. But has this law of *histological gradation* ever yet received a definite expression in language? Never. No physiologist has succeeded in upraising the shapeless mass into the luminous eminence of an undeniable verbal proposition. It will be accomplished. If species meet in *genera;* if genera, amid manifold outward diversities, centre in the type of the *order;* if orders converge in *families*, and families range into sub-kingdoms; if, in the growth of the mammal embryo, zoological types be assumed, though only transiently and foreshadowingly; if, in the fossiliferous series, there be traceable a chain, though its links be numerously broken, and its linearity interrupted; if there be, indubitably demonstrable, a serial homolotypism in "limbs"—the outward and visible members of the body;—why should there not be graven on things more deeply hidden --the internal vital organs—the impress of the same seriality?

But the critic, arrogant in the fog of a little German lore, will exclaim —"Why! the 'teleology of organs' conveys a full expression of such an idea. The fin of the fish is the teleologic antecedent of the arm of man —the same organ, modified according to the exigencies of special demands: *that* is the 'teleology of organs!'" It is *not* the idea of seriality which is now to be propounded. The doctrine of histological development lies at the foundation of all phenomenal externalities. It preserves the constructive symbol of unity beneath the deceptive exterior of varieties. It is the living clue of the Maker's finger, pointing the path through a world of wonders. Let it be travelled, but travelled with the humility of earnest science.

Every vital organ is the sum of component units, which arithmetic cannot count. The units, marshalled in a fore-planned order, issue in a premeditated action—the function. In the totality of the organ, the constituent integers are lost: just as the several organs forfeit individuality in the unity of the entire animal body. The doctrine which recognises a singleness of intention in the manifold elements of which the body of an individual being is fabricated, concedes in essence the doctrine which sees, in the entire sphere of animality, a constancy of purpose, a conformity of architectural style. Think of a four-footed animal without lungs, or of whose organization a liver formed no part! Is not the mind instantly seized with the idea of its impossibility? The supposition of an animal destitute of an organ essential to the *sum* of the organism, is as offensive to science as that of a planet in the orrery which, in its revolutions, sets at nought the physics of Newton. The sense of the *necessity* of a certain and definite *order* in events, a consistent conformity in the method of things, is as strong in the former example as in the latter. But all this is contemptible from its familiarity. It is one of the platitudes of vulgarized knowledge. Yes. But there are depths below this charted surface into which the plumb-line of science has never yet descended. Can the geologist make answer to the question, *why* the first mammals of the earth, the famed marsupialia of Stonesfield, appeared at a particular spot in geographic space, and a particular epoch in geologic time? Rea-

soning from final causes, he could only say, the "conditions" favourable to marsupialian life culminated in that period. Does such an observation add one single substantive fact to the treasury of real knowledge? Not in the least. It is the shadow without the substance of reply. Ask, in the phraseology of Dr. Alexander Braun,* *why*, at the age of six or seven years in human childhood, the phenomenon of rejuvenescence of the teeth occurs? Physiology is impotent in presence of such a question. The conjectural reply would speak of "conditions," advancing nutrition, period of growth, &c. The querist remains unsatisfied. The teeth of the second dentition are of a higher order of growth than those of the first. The permanent mark an advance in organization upon the temporary. But the former were not evolved by a transformation of the latter. The temporary were not first subjected to a reducing crucible, in order that their incandescent dust may be moulded into the permanent growth. In the structure of both tiers, there are exhibited conspicuous affinities. The identity of the *plan* on which both series were constructed admits not of a moment's denial; and yet both arose out of a common centre, from a common basis, as perfectly independent growths. The second in seven years after the first. This is not "rejuvenescence." The first did not *renew* the term of its life by, and in the evolution of, the second; nor was the second the product of the dissolution of the first; it was not a *redivivescence* of its former, but a new creation. The word "rejuvenescence" is admissible only as a poetic designation of a large class of natural phenomena. It is doomed to mystify and misguide, as a philosophical expression. It is chargeable with the *suggestio falsi.* With what consistency can it be said that the lungs of the frog are *the* gills of the tadpole rejuvenised? In fact, the doctrine of rejuvenescence is essentially synonymous with that of the transmutation of species. If one organ can grow out of or can be transfigured into another, there can be no valid reason for rejecting the hypothesis of the transmutability of species.

What is the difference between the idea of rejuvenescence as applied to the lungs of the amphibia, or to the teeth of the child, and to the *neider-blatt* and the *hoch-blatt* of the Berlin botanist? If the cotyledons are rejuvenised (reproduced) in the high leaves, the act is one of transmutation. But are not the phenomena susceptible of a more intelligible, if not more exact, analysis? Could the act of the inflorescence in a plant, which is the crowning event of the year's cycle of actions, be accomplished, if *not preceded* by all those under which the leaves are produced? Could the permanent teeth be evolved without being antecded by the temporary, or are the external gills *necessary* preparations to the development of the lungs in the amphibia? Are the caducous branchiæ the *progenitors* of the lungs? Are they not a single factor in an assemblage of agents conspiring to generate new physiological conditions, amid which lungs are perfected, if not formed? These physiological positions are really parallel. To accept the idea of rejuvenescence is to receive a euphonistic but hollow word; it presents to the mind nothing that is substantially tangible; it awakens a beautiful but bodiless vision. What more than a graceful fiction is there in the assertion that in cosmical transmutations fishes prepared the earth for, and were repro-

* Botanical and Physiological Memoirs, Ray Society, 1854.

duced in, birds, the second vertebrates in the geologic series? This is not severe science; it is sickly sentiment, because worthless.

The phenomenal sequences which occur in a single vital organism during the progress of growth, are not readily comparable with those which mark the advance of a series. In the case of the single individual, one event is traceable into connexion with the next. The thread of causation can be followed; such events are parts of a whole. The presence of the permanent organs of the body *imply*, with all the force of a physiological necessity, the preparative foregoing of the disused parts of the fœtal phase of life. Here there is an intelligible dependence, as well as a sequence, of events; but in individuals of a series, the *connexion* of events cannot be established. Who can venture to affirm that the fishes of the first were *parentally* related to those of subsequent periods? In the extant scale of animals, it is easy to suppose the existence of birds without reptiles, or reptiles without fishes. *But when the series is completed*, the symbols of consecutive conformable seriality are indubitably discernible on each successive link. These symbols are not written merely upon the outward surface of the entire animal body, but upon the inmost elements of each constructive organ.

Contemplate the kingdom of animality under the character of a triune system. In animal organization three classes of parts are distinguishable; they are anatomically distinct, but physiologically inseparable. The system of the *exterior solids* is descriptively, but not chronologically, the first; that of the fluids is the second; that of the *interior solids* is the third. Let three distinct, but parallel, lines be carried throughout the zoological chain from man to the sponge; these lines will be unbroken! Unbroken constructively, histologically! They break only where they terminate. Each of these lines is divisible into links or stages. The stages or gradations of one line *coincide in place and time with those of the two others!* The viscera, the productive glandular organs of the body, constitute one series; the fluids, the next; the organs of animal life (Bichat) the third. The visceral series is resolvable into several component lines, of coincident progression; the fluid and that of the exterior solids are reducible probably to the same number of lesser but co-ordinate series.

At the present stage of this exposition, it is quite immaterial to inquire what may be the order of precedence among these three systems of parts; when that question shall have been satisfactorily answered, another *principium* will have been added to the true laws of organization. Faithful demonstrative history must precede the mental act of interpreting. The *modus in rebus* is an after discovery, the product of judicial induction. The *fact* that along the paths of organic progression, whether tracked downwards in the direction of forms of life, which successively simplify, or upwards in that of those which ever multiply in completeness, certain great steps, signifying ascent or descent respectively, are periodically encountered by the anatomist, is a remarkable but clearly-written chapter in the science of comparative anatomy. But the signals of ascent or descent of *one* of the three classes of parts of which the sum of animal organism consists, run parallel with similar signals of change in the other constituent systems of the animal frame. What is the physiological

meaning of this *coincidence?* Has this question ever before been clearly propounded in physiology? Never.

It is not solitary *events* in the lineal families of living beings which demand to be recorded in history. The *desideratum* in philosophical physiology is to know *why* certain grand movements in the *standard* of the animal organism are experienced at one and the same time and place by several of its organological elements. Without such information as that implied in this inquiry, comparative anatomy can never attain to the consummation of a science, to a knowledge of the *material* laws which preside over the process of organogenesis. Thick events crowd into *nebulæ*. The clear sight of gifted genius is required to resolve the mist into its intelligible elements. The *fact* that an event does really happen is first to be achieved; demonstrate first, then generalise.

What signify the following illustrations? The *Amphyoxus* is the lowest vertebrate; it is furnished with every vertebrate organ but the spleen. This fact, stated as an isolated event, has no significance; it means nothing; it has no value in science. *Connect it with this extraordinary fact, that the blood of the Amphyoxus, though corpusculated, is colourless!* Now travel, but with circumspect and vigorous logic, along that path of thought, towards which this single but remarkable illustration so significantly points. Is the blood colourless *because* the spleen is absent? Standing apart, this question admits of no certain answer. Present it in a reversed form; is the blood colourless in any known animal in which the spleen is present? It is.* Then the predicate of the syllogism is logically inevitable, therefore the spleen is the generator of blood-pigment. The question demands a far more extended examination.

Every animal body is composed of two distinct and separate classes of solid organs. 1. Those of animal life; 2. Those of organic or vegetative life (Bichat). The blood stands intermediately between these two classified moieties of the organism. The viscera make the blood, and the blood makes the solid systems of animal life. Whether this relation be wholly or only partially true, it was formerly argued at length that every advance in the animal series which occurred in the nervous and muscular and sensual systems, was marked by a contemporaneous change in the chemical and morphological characters of the fluids. A new meaning was thus imparted to known events. A novel doctrine of animal seriality was unexpectedly established. It became incontrovertibly conspicuous that the sequences of organic development were directed by immutable laws, lying deeper than the surface of things. But is it possible that the jurisdiction of such laws can be limited to the systems merely of animal life? Far from it.

The physiological connexion which subsists between the visceral systems of the organism and the fluids, throughout the chain of organized nature, is a perfectly untravelled tract in comparative physiology. It is thickly planted with the choicest fruit-trees. It is a virgin garden, whence may yet be drawn votive offerings acceptable on the altar of science!

* It will be afterwards proved that the colour which occurs in the nutritive fluids of the vertebrate animal is totally dissimilar, if not chemically, assuredly morphologically, from that which characterises, with one or two exceptions, the blood of invertebrated animals.

The *exterior solids* of the body consist of the muscle, nerve, skeletal, tegumentary, and sense-series. Every index of standard change inscribed on either of these series, histologically, is *preceded by equivalent* marks of change in the system of the fluids. It is erroneous to suppose that the nutrimental, ministering fluids of the body represent a *single* incomplex organ. They are constituted of manifold elements. They perform different and varied functions. They should, both in number and office, represent a congeries of organs. The system of the floating corpuscles would denote a separate organ, the albumen another, the fibrine a third, the salts a fourth, &c. There is traceable in the agency of each principle or element a linear individuality of direction.

It follows, from this separateness and independence, that one or more of the elements may be withdrawn without destroying the physiological unity of the remainder. The same law precisely is applicable to the instances of the solids. The increment or decrement of the vital organism on the standard scale is respectively accomplished in one of two modes. 1. Increment is effected by the *creation* or the superaddition of a *new* organ to the systems of those already present, decrement being the converse subtraction; or, 2. An organ, or principle, or element already existing, is raised in the series by the homogeneous involution of its constituent parts; the added portions being more or less structurally identical with the pre-existing substratum. This is simple growth, *to which, however, there is an organic limit.* For the latter mode there are no marked variations of type; the degree only is multiplied. This law of graduated progression, by the superaddition of similar or dissimilar elements to a typal basis or nucleus, affects as deeply the organic principles of the fluids as the grosser organs of the solids. Fibrine, in the form in which it *first* appears in the scale of the fluids, is as different from fibrine in the phase of its highest preparation, as the lowest muscle-cell is from the highest. Albumen also exhibits a similar scale of graduated composition. It is of the utmost importance to the future progress of physiology to understand that *secreted products* differ with the machinery by which they are elaborated. This is self-evident. A simple organ, reacting on simple fluids, must obviously produce results of corresponding simplicity. It is materially and chemically impossible that the bile of the lowest animal can agree in composition, either in the quality or number of its constituents, with that of the highest. The incomplex, non-nucleated bile-cell of the echinoderm, or the annelid, in presence of the blood of the mammal, could not so marshal and rearrange the affinities concerned as to produce echinodermal or annelidan bile. Discordance between the means and the end jars on the reflective mind at every step. What conclusion does this common sense reasoning constrain? It seems irresistibly evident that an isolated, detached, single event *cannot* happen in the history of the organic elements. A new organ is invested with new functions. It supposes creative antecedents.

Arbitrary events, like arid facts, are utterly infertile. Until utilized, they are useless lumber in the archives of knowledge. The blood-proper system, as formerly announced, first occurs in the zoological scale at the echinodermata. *Why* does organizing nature, at this particular step in the ladder, enact this grand effort of a new creation? Is not the *creating*

of a *new organ* as great a marvel, a miracle, as the fiating of a *new animal* into being? Yes, most reverently! But stop!—Is this act of creating a new living *organ* illustrative of the doctrine that a something has arisen out of nothing? and that only by the interposition of the invisible will of an invisible Artificer? Human reason turns away with sorrow from the impious littleness of such a question. Is such an event marked by the occurrence of no other *connected* events in the organism? If it be not, physiology is not a science, and organization is not under the governance of the principle of typal progression. The antecedents of a material event must be material. A grand organic power cannot arise and exist in the living body, without leading to sequences also material. Every phenomenon of growth or evolution must be the *product* of anteriorly operative conditions. What conspired then, in the instance of the echinoderm, such that a new system of fluids came into existence? It is at this class that the visceral cavity, charged with the chylaqueous fluid, is first closed into a shut, independent space. At this stage the open fluid or phlebenteric series terminates. The biliary system augments in proportions. It lines the large tracts of digestive surfaces presented by the cæca. Every portion of chyme in these cæca must traverse the living walls osmotically in order to gain the splanchnic chamber. In this passage it blends with the product (bile), and receives the biochemic impress of a living solid. Thus is elevated its organic standard; thus, too, is raised its nutritive value.

It is capable of doing new work, of *creating* new systems. A changed arrangement in the apparatus of the *interior* solids, the digestive viscera, entails on the chylaqueous fluid an improved composition; the latter ministers to the apparatus of the *exterior* solids, and unexpected results are accomplished.

Let the foregoing example suffice to illustrate the methodological principle, which it is designed to follow in these investigations. Every phenomenon annunciative of an organic change in the anatomical place or chemical composition of *the fluids*, must have been preceded and conditionated by indications of altered standard in the productive apparatus of the *interior* solids, just as it is succeeded by symbols of advancement in the system of the *exterior* solids. But is not such a proposition liable to the charge of vicious circularity? If the viscera generated the fluid, and the fluids produce the muscles and nerves, &c., what formed the viscera? All physiological reasoning partakes of the faults of the circular logic. The cycles of phenomena are so labyrinthically interblended, that neither a detached beginning nor a non-re-entering result can be discovered in the mingled rounds of vital operations. The gastric fluid may be said to be the beginning or the end of a thick tangle of events. 1. It is produced by the blood—the end. 2. It constitutes the initial term of a succession of actions, of which the blood is the final product. This is undeniable. Henceforth, nevertheless, the organic sciences must march along the highway of demonstrative truth. To establish the *method* of sequence, to prove that events happen *in connexion* with others, is to enhance the value of all, and clothe them in the garb of a higher significance. It indicates a new path of investigation. It suggests a principle for the classification of phenomena, from which comparative anatomy will receive an ennoblement of rank.

The systems of the *interior or visceral solids* are reducible into several apparently independent and unconnected series. The *alimentary or digestive apparatus* enjoys the widest range of distribution: it begins at a lower point in the scale than any other system; it is present when the rest are either entirely absent, or present only in a dubitable rudimentary form. It will be the object of this investigation to determine whether the phases of mechanical type, of general conformation, which this apparatus discovers as the succession of classes are tracked upwards, are accompanied by co-ordinate symbols of elevation in the system of the fluids. Phenomena, hitherto deemed incomprehensible, because viewed in their irrelated isolation, will thus be drawn within the cycle of necessary and orderly sequences.

The *biliary system*, followed throughout the phases of its evolution, but with constant reference to the development of the fluids, will conduct to a knowledge of new phenomena in the serial history of organization. The liver is wide-diffused. It is said to be a conspicuous feature of the lowliest organisms. It must exert a preponderant influence over the generation and organic standard of the nutritive liquids. The search from this exalted point of view will lead to unexpected results.

The *organs of breathing* constitute another visceral element of universal presence. In the sphere of its distribution, and in importance, they rank next to the biliary. Examined in relation to the fluids, they will point to a new sphere of thought.

The *renal system*, in its reaction upon the liquid nutritive media of the body, offers a special but fruitful line of study. It has never been contemplated from the eminence of this novel conception. Insects and arachnida are the only invertebrate animals in which it exists. It is not present even in the cephalopod.

The *reproductive organs* fall scarcely within the category of the fluid-making series. They belong more properly to the sphere of the exterior solids. They are rather products than producers of the fluids. Every *germ-cell* (ovum) is histologically identical. The sperm-cells are as numerously varied as the *species* of the animal series. Unequals, added to equals, give unequals. The germ-cell of every species numbers the same constructive elements. The vitelline capsule and its granular yolk substance, the germinal vesicle, and the germinal spot, *are present in all.* Wherefore this uniformity? How is it that unequals produce equals? How is it that the zoophyte, at the furthest extreme of the kingdom, whose fluids are of the least complicated order, can form an ovum which discovers an identity of structure with that of the mammal? Does not this illustration at once destroy the doctrine which claims for *the fluids* a graduated scale of Development? It does so apparently, but only apparently. An *ovum* is a nucleated cell. The simplest and lowest nutritive fluid is capable of generating so simple an organ; and only such elements of the fluids, in the instance of the higher animals, are withdrawn in the production of the ovum, as are required to form a simple cell. Familiar facts, concatenated under the guidance of a new theoretic scheme, acquire a new value.

The *spleen* is emphatically a vertebrate organ. Among the invertebrate it has no equivalent or representative; it is not required. It fulfils

an office for which no demand exists in the inferior half of the organized series. It bears an intimate relation to the blood-producing actions. As formerly stated, it is absent in the lowest fish—the *amphyoxius*. Its absence is coincident with the non-existence of colour (hæmatosine) in the blood! The *correlation of events enhances their individual values!*

The *pancreas* is said to be present in gasteropods and cephalopods. No other class among the invertebrata is even supposed to possess this organ. It exists in every vertebrated animal. The truth is, that no representative of the pancreas can be found in the invertebrate organism. Let the philosophic reader pause to reflect over *the meaning* of these wondrous epochs in the history of *serial organo-genesis*. Are they not *creative* epochs? At successive stages has he not witnessed the "spontaneous generation" of a system of living organs which, in the chain of organized beings, had no existence before? Is he not startled into admiration? Does he not feel that a cloud is being banished from the horizon of his view? But let him well remember that these are not acts of creation *ex nihilo*. The building materials are first prepared, *the act*, the gross, visible consummation, is then accomplished. Nothing is here *transmuted*. No fusion or confusion of two beings or two organs in order to make a third. Nature's method consists in the gradual accumulation of such physical circumstances as are essential to the production and maintenance of a new order of things. A complex assemblage of casual conditions are seen to converge upon an undeclared end. The end is at length announced, and the act of creation is accomplished. The march of the preparatory processes is obvious to the eye. The mechanism of the creative act is literally legible to the understanding.

Now, in what essential respect does the organic fact of the *first appearance* in the scale of serial organization of a new and independent system of living *organs*, differ from that of the creation in geologic time and place of a new genus or species of animals? Only in this: that in the former instance, the mechanism of the act is resolvable by the analytic power of science; while, in the latter, no approach can yet be made to a knowledge of the productive conditions. *Faith* in the uniformity of natural laws constrains the most reverent thinker to believe, that between the two acts of creation there must obtain an intimate analogy. It is impossible to imagine that events so analogous can be accomplished by irreconcilable modes.

The successive superimposition of elements or organs to the system of the individual in the upward march of growth, is really, in mode, and type, and essence, the same as the consecutive addition, in a conformable manner, of new members, independent beings, to a series of orderly types. In the instance of the specific chain, however, the cumulative method, which is necessary in the perfecting of a single being, from infancy to maturity, is not an irreversible method of creation. The highest or a middle member indifferently may appear first. Each member is a detached and independent individual, although constructed in intimate conformity with those juxta-posed. No two, though joined by similitude of model, are mutually convertible.

Imagine two ideal beings; in one is figured an invertebrate, in the other, a vertebrate animal. In what organic particulars are they diverse?

The organism of the invertebrate animal is compounded of a digestive, biliary, reproductive, and respiratory systems; in the case of the vertebrate type, *three* great classes of organs are superadded to the number of those required to form the simpler model, the renal, pancreatic, and splenetic series of organs suddenly rise into existence. Could such material machinery breed arbitrarily in the vacuous womb of nihilism? Are they a mystic progeny without progenitors? Why is it that science has never moulded into intelligible language these most natural questions? Is there not in this *mode of working* distinguishable, in the clearest manner, the principle of progression, of typal seriality, of rising from things little to things great, from lower to higher standards of organic mechanism? There is, beyond all doubt. But it remains to demonstrate, by reference to the acknowledged data of descriptive anatomy, what the master demonstrators in that science have never suspected, that a new viscus added to the body, or an old one modified, is the symbol of a cycle of changes necessarily flowing from, and consecutive to, that one act of creation. Pursued under the brilliant light of this novel methodological conception, the details now to be studied will add another triumph to the first principles of organic science.

Let first be studied the serial history of the *digestive system in relation to the fluids*. In the stomach, the first and the last acts of nutrition meet. In its simplest phase this organ is a bag, perforated at the bottom. On the walls of this bag *all* the visceral or gland organs (except the reproductive) of the body are commonly supposed to be situated. It is in direct communication with the splanchnic cavity, which lodges the nutritive fluid. The glandules which are distributed over its surface consist of cells filled with oleus molecules. These cells are *not* nucleated; they should be described as glandular capsules. In them are generally supposed to be united two independent offices, that of the gastric juice and that of the liver. The model of this form of digestive system occurs in the hydroidan and actinian zoophytes. All the vital preparation which the fluids in these classes are required to undergo, consists in their admixture with the secretion derived from the walls of the stomach alone. Nothing else is added to the blood-making apparatus. The fluid enters at once into the cavity of the body. It is there aërated. It is then qualified to react upon the solids for their nutrition. Could so simple a vital machinery, viewed apart, considered alone, ever receive a consistent physiological solution? It is by calculating it as the last and lowest member of a descending series, or the first and simplest point of an ascending line, that the *meaning, the possibility* of its simplicity can be shaped into the consistent form of a reasonable problem.

The fluids consist only of a weak limpid solution of albumen. They display only the faintest tendency to corpusculation. Now, reflect upon these marvellous phenomena, first in the chronologic order of their occurrence, then in their reciprocal connexions:—(1) a simple system of interior solids:* (2) simple fluids; (3) simple exterior solids. That is the

* I have re-examined with great care the reputedly biliary character of the pigmented cells which are distributed over the *internal* surface of the stomach in *Hydra* and *Actinia*. Various species of the latter were subjected to analysis, with special view to the solution of the question discussed in the text. The tissues are most transparent, and therefore most favourable, in *Lucer-*

march of events. Catechise more minutely the first members of the triad. The stomach alone, of all the inextricably complex organs afterwards added, in the higher grades of the scale, to the system of the interior solids, is here present. Is that literally the case, or is the gastric open-bottomed sac of the zoophyte the *whole alimentary system* of the higher animals in epitomized miniature? Does so simple an apparatus in secret and invisible littleness, comprehend not only the liver, pancreas, spleen, &c., but the small and large intestines, and their associated glands? If so, physiological or serial simplicity can only mean minuteness of size. The lowest and simplest organism, in the conventional sense, must be synonymous with the "highest in miniature." This illustrates the confusion of ideas, the transmutation of thought, by which the progress of discovery is retarded, through lack of clear definition as to the sense in which words are to be employed. Attempt another interpretation of the facts. Nature attains the ends of "simplicity" of structure, by gradually and cautiously subtracting the inessential from the essential elements of an organ. The nuclear type is preserved. When this last is withdrawn, the last teleologic idea, as well as the substantive reality, of the organ disappears. But is there not a point, in the descending scale of animality special and proper to each separate element or system of the organism, at which, the process of deducting non-essentials having been carried so far, the last *essential* remnant, the archetypal essence of the organ, finally and completely vanishes from the scene? or, reversing the direction of the serial march, a point at which every organ has *a beginning!* Examine, again, the stomach of the zoophyte from the vantage eminence to which the mind has been raised by the preceding discussion. If every organ concerned in the formation of the nutritive fluids, in the case of the higher animal, were centralized, though only in the guise of their initial rudiments, on the walls of the actinian stomach; if one nucleated cell performed the office of a liver, another represented the pancreas, another the spleen, &c.; it follows, with undeniable certainty, that the fluids resulting from the elaborative secretions furnished by such a compound organ, would correspond *in quality*, in the number of their elements, with the fluids of the *highest animal*. Equals produce equals, complex conditions determine complex results. This is really the doctrine taught by every living systematic physiologist. The mind is chained by preconceptions as false as they are confused.* Why should not an integral *organ* of an individual

naria Auricula, not uncommon on the coast of Swansea. I have arrived at the conclusion not without thought, that when the alimentary system *terminates* at the base of the stomach as exemplified in the Hydroid, Asteroid, and Helianthoid Anthozoa, a biliary system *does not exist*, has not been created. It will be explained at a future time, that in the lowest radiata it is extremely difficult to define the difference between the true gastric glandules, which in their anatomical *locale* are strictly limited to the stomach, properly so called, and the biliary glandules, which as constantly are distributed over the walls of that segment of the intestine which succeeds the stomach in an antero-posterior order. If *a liver* could grow in or on the parietes of the stomach, nature would approve herself guilty of an incomprehensible outrage upon the constancy of her own principles. Every organ has an inviolable *anatomical locality*, just as every species has its *serial place*. To cause a liver to grow on or in the stomach, would be the same organic victory *precisely* as to transmute a species!

* The "homology of organs" is nothing but "ascensive development" of organs. The *anatomical place* of the organ is never transferred. Amid a thousand variations in the outward physiognomy of organs, the law which demands *constancy in the relative position* of parts remains absolute.

being, just as much as a species of a genus, have *a beginning* in the organized series? The first appearance of a new individual or group of beings in geologic chronology is signalized as a mystic era, annunciative of the omnipotence of Creative Power. The first appearance of a *new organ*, as wonderful and perfect as an example of organic mechanism as any separate individuality, is viewed with indifference, and reluctantly chronicled in the annals of science. In gazing at events *at present* utterly inapproachable to the human understanding—the occurrence of species *in time*—the unresolved nebulæ of cosmic chronology—he culpably overlooks the *comprehensible* phenomena of creation; comprehensible because legible in their attendant conditions. In the instances of new organs, the steps of the creational processes are literally traceable, *seriatim*. The stomach of the zoophyte is then the *first form* or shape under which the alimentary system of the interior solids begins in the animate series. But let it be emphatically stated, it is only the incipient representative of the stomach proper; it is not a compendium of all the alimentary machinery of the higher animals. If it be only a stomach it follows, with irresistible certainty, that the stomach proper is the first created link in the alimentary chain—the first letter in the continuous series of the alimentary alphabet—that the duty performed by it must be the lowest expression of the function discharged by any other stomach proper, not of the entire digestive system of more completely organized species. Here, then, "simplicity" of office is synonymous with *singleness* of action. Singleness of structure is the obvious inference!

The method of evolution, as it applies to the alimentary system, consists in the successive addition of *new* segments antero-posteriorly from the stomach to the *colonic* intestine. The distinction between the small and large intestines is not clearly defined in the invertebrata. This is the serial law which presides over the development of the digestive canal. Let it be further considered if, in the zoophytes, the stomachal extremity or segment only of the digestive canal be present, it is only the *gastric* agency which can be exerted upon the fluids. The product of such agency is the correlate of that of mere stomach digestion in the higher animal. This method of explaining the low type and inferior standard of composition displayed by the lowest order of fluids, is at variance with no known fact in serial organization. When the true natural history of the proximate principles shall have been written, when it shall have been proved by chemical demonstration, that there obtain, even in albumen and fibrine, *grades* of vitality and composition, the question will be answered whether the *finished* nutritive fluid of the zoophyte is the equivalent only of the chyme of the vertebrated animal. Let this question be waived for the present. The proposition has been established that in the first and lowest class of animals, in which the material conditions of existence are the simplest, and therefore most intelligible, the *triad systems of the organism bear towards each other a definite, direct, and constant ratio!* The lowest pattern of visceral apparatus engenders the lowest type of fluids, and the lowest standard in the fluid series determines the most degraded class of exterior solids. Can it be otherwise?

The *alimentary system of the medusæ* presents an unambiguous advance upon the former. Cæcal prolongations are superadded to the central

gastric sacs a new organic place is created; a biliary system arises to occupy that place, yet not in an unequivocal form. The gastric canals of the medusæ are not the counterpart of the visceral cavity of the actinia. The former is anatomically continuous with the stomach, the latter is a separate chamber. In the gastro-vascular canals of the medusan, the contained fluid receives new impulses of growth. In the cavity of the body of the actinia it is at its last stage; it can rise no higher in composition; it was necessary to superadd the gastro-vascular cæca to the central stomach in order *to create an anatomical place* for a new system of organs—a biliary apparatus. The fluids could not be raised in standard without a resort to some such expedient. The fusion of organs is as contrary to law, as abhorrent from true science, as the transmuting of species; a new organ must, therefore, have a new locality; it is provided, a new agency is introduced, what are the physiological consequences? The fluids are raised in vital and chemical composition! The degree in which the fluids are elevated is proportionate to the advance which may have occurred in the elaborative solids; the sequence is necessary and co-ordinate. How simple, how perfectly comprehensible the *mode* in which the act of creation is accomplished! In zoophytes and medusæ the alimentary system contains, bounds, the nutritive fluids. In all animals above this limit, these fluids escape into separate and distinct recipient canals or cavities. The medusan organism consequently marks the upper limit of true *Phlebenterism.* The physiological sense implied by this word is far more clearly expressed by the word *gastro-vascular.* About the former designation there hovers an elegant Grecianism, about the latter plain common sense.* By the word *Phlebenterism,* it is meant that the cæcal tubes appended to the stomach prepare, contain, aerate, and distribute the nutritive fluids of the body. In the MEDUSÆ all these functions severally and collectively are undoubtedly discharged by the gastro-vascular canals. But the word was never intended to be applied to this class of animals. It was invented only to express the anatomical peculiarities which distinguish the alimentary system in certain classes of gasteropod molluscs, exemplified chiefly by the Eolidan and Doridan families. Between the gastric processes of these molluscs and the corresponding diverticula in the Asteridæ among the echinoderms, Pycnogonidæ among the crustaceæ, Planariæ and Clepsinidæ among the annelida, the Trematodæ and Cestoidea among the entozoa, there obtains this radical difference. In all the latter examples, the cæca bear the biliary organ *in their walls*; in the Eolidæ and Doridæ a separate liver is provided.

The tubular appendages into which the stomach in these nudibranchians is multiplied cannot, therefore, prepare the fluids further than what is implied in the act of digestion. In minute anatomical structure the digestive cæca of the Eolidæ and Doridæ are consequently not homologous with, although analogous in figure to, the corresponding appendages in the digestive system of the Asteridæ, Planariæ, Pycnogonidæ, &c. The resemblance is only anatomical. Physiologically, they are irreconcileable.

* The word *Phlebenterism* originated with M. Quaterfages. While engaged in the pursuits of natural history, in company with M. Milne Edwards, in the years 1843 and 1844, he published a paper in the Comptes Rendus, for July 15th, 1844, in which he noticed a group of gasteropod mollusca, for which he proposed the distinctive name of *Phlebentera.*

The Phlebenterata of M. Edwards and M. Quaterfages *fail*, then, in the alleged function of sanguification. This fact is in itself enough to prove that their fluid contents cannot be a *completed* nutritive medium. But the very idea of Phlebenterism is destroyed by this irrefragable fact—in every example (the medusæ and zoophytes excepted) of supposed Phlebenterism, whether drawn from the lowest crustacea, echinodermata, annelida, entozoa, or mollusca, the gastro-vascular appendages *float in and are surrounded externally by a layer of fluid.* In the nudibranch molluscs and Pycnogonidæ, it is true-blood, because circulated by a heart in a definite orbit of movement; in the Planariæ, Trematodæ, and Asteridæ, it is a real chylaqueous fluid, because, though occupying the same anatomical position, it is *not* circulated by a heart. It moves to and fro under the rhythmic contractions of the alimentary diverticula.

Suppose the contents of these gastro-vascular cæca to be a nutritive fluid, while in such situations, how is it to be aerated? Necessarily, if at all, *through* the stratum of fluid by which each cæcum is embraced externally. Though this precise method of reasoning against the tenableness of the Phlebenteric theory has not been followed by Mr. Hancock and Dr. Embleton, they object on a ground quite as valid, that a true and very exalted vascular system exists in those very molluscs which M. Quaterfages has signalized as the most degraded.

But are those families, already enumerated, which are characterized by a ramifying alimentary system, coequal in organic standard because they are similarly constituted in the special feature of a vasiformly subdivided digestive apparatus? This question recalls the mind to the point from which this discussion originated,—that the organic standard of the fluids is directly determined by the serial grade of visceral solids. In the zoophytes and medusæ *all* the blood-making processes, inclusive of the respiratory, occur in the digestive system. No single sanguiferous action takes place external to or beyond the limits of this apparatus. *This* is true Phlebenterism. Nothing beyond and above this limit is unmixed Phlebenterism.

Let the law be here remembered, already emphatically proclaimed, that the simplest type of the system of the *interior solids* is coincident with the lowest standard in the fluids. View the phenomena again in their organic connexions—Do they not arm the philosophic anatomist and the vital chemist with a *novum organon?*

Proceed to a higher grade in the series—to the *Echinodermata.* An extraordinary event occurs: the fluids are impounded in a new, closed, and independent cavity,—a cavity which had no previous existence in the chain of animal life! Is this striking episode a causeless, arbitrary circumstance? Does it stand apart as an unconnected novelty in the organism? Impossible! It is raised in chemical and vital character as compared with the Phlebenteric fluids of the medusæ and zoophytes;—but *why* and in what manner? Examine the blood-productive solids. To an incomplex gastric sac, cæcal tubular appendages are superadded. A true biliary system then arises, but *not* before. These cæca cannot be a part of the stomach, properly so called. The presence of liver-glandules on their parietes is conclusive against their gastric homology. They are the foretype, in ambiguous rudiment, of that glandular blood-making

segment of the alimentary system of the vertebrate animal which comprehends the small intestines.*

The partitioning of the digestive from the fluid system gives to the latter individuality. It is the mark of organic elevation. No incident, in a dependent cycle of actions, can transpire without co-incidents or consequents. What are they here? A liver is added to a stomach—two blood-making organs! The fluids indicate, express the event. They betray the signs of new productive agencies. They acquire more definitely organized corpuscles. They experience an augmentation of proteinised principles. If they did not, no visible consequences would flow from the superaddition of a liver, though simple, to a stomach, itself of the lowliest type. The fluids are consequential. They denote the *second* member of the organic triad,—the second element in a tri-partite individuality. The fluid occupying the peritoneal cavity in the Asteridæ is the *initial phase* of the true chylaqueous system. *Everything* in the Asteridan organism corresponds with, and explains this initiality in, the fluids. The simplest biliary system is joined to the simplest stomach. If, under such circumstances, the fluids were organically complex, would not *reason* be outraged? In the ways of living, as of unliving Nature, there is reason. In the laws of the former, as of the latter, there is uniformity! Cannot, then, the physiologist foretell results, the conditions being given? If he cannot now, in this age of history, he will at a future!

But there is another event to relate:—*A blood-proper system intrudes itself on the scene.* Why? Because the productive, creative conditions are realised. The result intended *must* follow!

Here is another *act* of creation!—Could a true-blood system arise and live in a zoophyte or medusan organism? If there be *method* in Nature's creative scheme, it could not. Then, in serial organogenesis there is a *designed beginning* to *organs*, or systems of organs, as in serial zoogenesis to *individuals*,—to integers of individuality as to species!

Move higher. In the *Sipunculidan genera* the alimentary system augments in complexity. The fluids rise in a similar ratio! The corpuscles of the chylaqueous fluid acquire colour-pigment; the fluid becomes more thickly albuminized; a blood-proper trunk assumes a separate consequence; the nerve and muscle systems rise in importance! Can the force of these demonstrations be rationally opposed? Can the *facts* on which they rest be denied? Then does not the physiologist see that events, *creative* "*miracles*" in the history of organization, are really nothing but *necessary sequences!* One link necessitates as it conditionates the next. In Nature there is no solitarity of acts, no capricious, licentious singularity. Her operations, even the most hidden, are bound by the adamantine chains of inviolable law.

More onward still,—the *entozoa* forefigure the annelida. Every class of

* For some years I have noted the instances in which I have succeeded in discovering the presence of food in the discal sac, the true stomach of the star fish. In no single instance could it be proved that particles of *undigested* food passed into the interior of the cæca. Each of these appendages is guarded by a sphincteric apparatus which, pylorically, excludes every thing but *prepared* chyme from the chambers of the cæca. In the cæca the *second* act of digestion occurs, not the first. These facts are advanced in support of the doctrine that Nature does not locate organs in the organic machinery indiscriminately; the *place* of an organ is as absolutely fore-ordained as its anatomical elements, its specific structural type.

animals in the organized series, stands on a more or less separate basis; forming the bottom of a secondary diverging line, resting on one common to all. The entozoa are not definable as the natural continuation of the Sipunculidæ, and yet they constitute the lowest members of the annelidan series. In the scheme of the animal kingdom there is discernible no *literal* linearity. Here, accordingly, the wave of progress again descends. The blood-proper system present in the echinoderms disappears. The chylaqueous system resumes its exclusive prevalence. This descent is accompanied by the marks of degradation in the system of the visceral solids. The biliary apparatus sinks in anatomical characters. Its identity is scarcely determinable. The fluids exhibit inferiority. The alimentary system betrays faint traces only of the distinction afterwards to be declared between the stomach and intestines. The liver-system is of the humblest order. Look at the *annelida:* the stomach-proper is individualized; the intestine is stratified by a higher form of biliary system; an exalted type of chylaqueous fluid occurs; an unambiguously developed blood-system, provided with self-acting contractile vessels, appears on the stage. In the measure and standard of these successive phenomena, is there not inscribed the impress of proportionality?

The planariform entozoa and annelida are parallel in these particulars: both are destitute of a blood-proper system;* both exhibit a *ramified* alimentary system; in neither has the biliary system received a separate form, it is distributed over the parietes of the digestive cæca; in both, the nutritional fluids are of the lowest type. But the more highly-organized orders of the family of annelids denote a striking complication of machinery: a separate and independent blood-system arises; the fluids thicken, from the increased amount of the proteinized principles; the floating corpuscles mark an improved standard of structure; and, finally, the character of the exterior solids is raised. These events follow one another with an inviolable parity of march. Are they not inevitable sequences?

The *alimentary and biliary systems in the lowest mollusca* include still the entire sum of the blood-making solids. It is by their anatomical characters, by their serial degree, that the standard of the fluids is to be determined. The stomach is dilated, such that it may be distinguished from the intestine. The liver in every genus is situated upon the walls of the intestine. In the genera *Ascidia*, *Dendodroa*, and *Cynthia*, the liver is said to be absent.† This is an error: it exists, in all these genera, in form of a layer of glandules in the intestinal wall. In some orders, this

* The reader is referred to the 'Annals and Magazine of Natural History,' November, 1853, for a full discussion of the question which relates to the fluids of these classes. The views of M. E. Blanchard, which ascribe a complex blood-proper system to the parenchymatous entozoa and Planariæ, are there shown to be an utter misconception of what exists in nature. The beautifully painted system of bloodvessels which grace the last edition of the 'Regne Animal' afford an overdrawn view of the passages, bearing in these classes nothing but a chylaqueous fluid. In the chapter in the 'Annals of Natural History,' to which I have referred, I now think that I have assigned too large a share in the office of respiration, and probably too high an organic standard, to the *corpuscles* of the chymous fluid contained *in the interior of the digestive cæca*, in the trematode entozoa and planariform annelida. It is, however, certain that *the contents* of these cæca in some instances do undergo aeration. In *Aphrodita aculeata*, so express are the provisions for bringing these cæca into advantageous contact with the external water, that the question can scarcely be disputed.

† Article 'Tunicata,' Cyclop. Anat. and Physiology, by Rupert Jones.

segment of the intestine is thickened by the focculent development of the liver. It is the phase of the organ which precedes its centralization into a separate individualized form.

In *Chelyosoma*, the "cæcal tubes" distributed over the walls of the stomach are described as the liver.* If this be true, there can be no foundation for the law of *anatomical place*, by which organs are governed in development and distribution. The author's own dissections assure him that there prevails but *one* type of biliary apparatus in the tunicate molluscs. It constitutes a coating to the anterior extremity of the intestine. It discovers higher characters than those of the liver system of the annelida, which invariably occupies this situation. The distinction between the stomach and intestine in the annelida is only faintly traced. In the fact of the individualization of the stomach, the tunicate molluscs surpass the annelida. If the former are superior to the latter in the serial features of the alimentary systems, it may be predicated with certainty that they are superior in other characters.* Nature's career is inflexible. What are they? A complex organ of propulsion—a heart—is added to the apparatus of the circulation. By this single provision, a chylaqueous is transformed into a true-blood system of fluids. The duplicity of the fluid system of the annelids thus disappears. A circulation of blood is now first established, in the fulness of its physiological meaning. The cardiac centre has arisen. The fluid circulates with orbital regularity. The second and subsidiary system—the chylaqueous—has ceased in the series. The blood-proper is the only medium of nutrition. In the echinoderms and annelids, this system is provided with no trace of a muscular cardiac centre. Catalogue these symbols of advancing seriality. *Tunicata*—stomach and intestine separate and distinct—biliary organ unchanged in type, advanced in degree—chylaqueous fluid disappeared—a heart created—a true circulation of corpusculated blood established. Is not the *mechanism*, are not the *rules*, of organic creation perfectly orderly, and traceable to the last *material* conditions in this example?

An elevation in the standard, an increment in the working power of the visceral solids, simple, and two in number only though they be, necessitate an advance in a similar ratio, not only in the type of the apparatus of the fluids, but in the composition of the fluids themselves. The further the analysis proceeds, the more inconvertible does this law of parity of progression, in the component systems of the organism, appear. The *acephalous molluscs* contribute decisive facts to the cumulative argument. The stomach and intestines are more obviously differentiated. The liver swells in mass, is multiplied by tubuli, the heart rises in structure and dimensions, the bronchial apparatus assumes preponderant proportions, the measure of the respiratory office is augmented—an advance is accomplished in the standard of the fluids.

The digestive organs of the *gasteropod molluscs* are highly developed. The stomach is completed, formed. The intestines lengthen by convolutions. A new advance in the visceral solids is, at this stage, effected; a *salivary system* is superadded. Here, again, is the *first* unequivocal appearance of an *additional* integer in a pre-existent organism. Wherefore so tardy? Because the fluids required by an incomplex machinery were

* Article 'Tunicata,' Cyclop. Anat. and Physiology, by Rupert Jones.

themselves so little raised in the scale of vitalization, that a mere stomach and liver sufficed for their production; but in order to the gradual accretion of new parts to the sum of the body, the standard of the fluids could not be elevated, save by the provision of *additional* organs. The object could not be accomplished by the involution of the old. A *specially*-acting chemical power has now become essential—a salivary system is *created.* The liver assumes an enlarged and isolated character, and a symmetrical position. A heart subdivided into auricular and ventricular cavities—a distinctly-recognizable approach towards the organic definition of the vascular system. *Fibrin*, in an unambiguous form, occurs in the blood. The system of the *exterior* solids, as explained in a former paper, betrays the unequivocal signs of growth. But to the organism of the gasteropod molluscs a renal apparatus is also added in a decided form. This system is only doubtfully present in the acephala. Does not the physiologist perceive, with greater and greater brightness of intellectual vision, that, as the blood rises as a chemical compound, as its albumen and fibrin increase in relative amount, as its morphological elements betray the signs of higher organization, new productive elements, organ after organ, accessions to the blood-making capacity of the system of the *interior solids*, are being made at a corresponding ratio?

Extend the survey to the families of the *articulated series*. In *insects*, effective masticatory appendages initiate the digestive system. Mark the complex subdivisions which now occur—a muscular œsophagus—a crop (*ingluvies*)—a muscular gizzard (*proventriculus*)—a stomach (*ventriculus*)—an intestine—foreshadowing the vertebrate segments of the ileum and colon.

Is there not in all this a profound significance? *Every organ* which succeeds the digestive system in the order of occurrence of physiological acts, bears the impress of the exalted type of the digestive machinery. It is the centre of the system of the interior or blood-making solids. Around it cluster salivary glands, Malpighian or biliary tubuli, affecting a *pyloric* situation, a renal system connected with the cloaca. This machinery of visceral solids is the physiological basis upon which rest the extraordinary superstructures of the respiratory and circulatory systems of insects.

The *crustacea* contribute confirmatory evidence to the consistency and truth of the great rule of serial organization, which it is the object of this memoir to inculcate. The *alimentary canal* is subdivisible into a gastric segment with its intricate appendages; the stomach is completely organized; the intestine, though short and straight, is similarly formed. The liver in this class enhances in organic value. In the lowest crustacea, it constitutes a glandular, follicular stratum, enveloping the intestine; in the highest it consists of two glandular masses, composed of more or less remote cæca bound together. The renal organs of this class are imperfectly developed. Connect the preceding facts with the history of the fluids. They are inferior in standard to those of insects—why? The visceral apparatus is less completely organized. The heart, however, is well formed, subdivided into cavities; vessels are constructed, highly-organized corpuscles float in the blood, and fibrin is one of its proximate constituents.

Indulge for a moment in the luxury of a retrospect over the historic

sketch now summarily drawn. The lowest invertebrate animal is provided *only* with a stomach. It is the organic centre of the entire body. Every other element, fluid and solid, is measured and apportioned by this primal organ. The fluids occupy the lowest extreme of the scale. A liver is next added. The fluids rise one stage. An intestine is appended to the stomach; the liver augments. The fluids multiply in the number of their constituents, improve in the quality, and increase in the amount of the old ingredients. Additional chemistry is provided in the salivatory and renal organs. The products of the agency of the latter are announced by indications of advance in the standard of the fluids.

One great sanguiferous process remains to be considered before completing the survey of those phenomena of progression in the invertebrated animals, which prove incontestably that *organs*, as well as *species*, obey the grand creative ordinance, that all things living shall *rise* from what is simple to what is complex, from what is low to what is high. The *respiratory system* differs from the ordinary glandular organs in this extraordinary feature, that it knows no variations or differences *of quality*. The stages of *its* progression are those only of degrees. Can this be said of any other sanguiferous gland? The question cannot be confidently answered. Secretions, products, graduate in composition as organs progress in structure. But there must be a point at which *bile* is bile, or *not* bile. If it be bile, the cell which formed it is a *liver*-cell. The bile of the echinoderm may be composed of only *one* or *two* of the numerous ingredients of which the same secretion in the vertebrate animal is constituted. It is less by varying the essence, the basilar constituent of an organic product or of an organic apparatus, than by adding to, or withdrawing from, the number of the component elements, that Nature's workmanship rises or falls in the scale. Of respiration only thus much can be stated—the lowest nutritive fluid absorbs oxygen, and emits carbonic acid; the highest only does the same. The difference lies in the amount. A simple fluid, composed only of a very dilute solution of albumen, demands the lowest grade of the respiratory action. How could it be otherwise? Carbonic acid is the product of the molecular metamorphoses of organic tissues and organic principles. If these latter are of the most degraded order, this especial function must be at its minimum.

The reasoning admits of no opposition. Then in writing the serial history of the respiratory organs, nothing but variations (endless!) of proportion and outward shape have to be recorded.

This absolute principle with reference to the function of respiration may, however, be propounded. Whatever be the external form of the organ upon which this function devolves, the amount, the *meter* of the function, is directly proportional to the organic standards of the fluids. Complex fluids evolve a large amount of carbonic acid, and take in a corresponding value of oxygen; simple fluids a small. That is the *serial law of respiration.*

Given the standard of the viscera, to determine the composition of the fluids? Given the fluids, to estimate the measure of the respiratory function? The problems are practicable. They breathe, however, the accents of a science which sleeps to glorify a future age.

Advance to a general view of the conditions which pertain to the for-

mation of the fluids in the *vertebrated series.* Every genus of invertebrate animals begins at a low species, and rises to a high. Every genus of vertebrate animals does the same. Fishes have their lowest and highest types—birds have theirs—mammalia have theirs. The animal kingdom does not represent *one* straight, continuous series, travelling and multiplying along one rectilineal path. The true classification of animals will be eventually founded upon this principle. One highway traversing the distances, intervening between the lowest and highest species, will be defined. From this *common* highway—common, that is, to species and genera—secondary by-roads, ending in unconnected extremities, will be found to diverge.

This figurative conception expresses the principle on which the animal kingdom should be classified. The vertebrated are a natural continuation, in every philosophic sense, of the invertebrated series. The same arithmetic laws preside over the multiplication and development of the blood-making viscera. The principle of progressive serial development is indubitably legible in the organs and the chemical processes which precede and elaborate the organic, proteinised principles of the vital fluids. The fluids are the sum, the product, of the organic actions by which they are preceded. The interior solids are the seat of such action. This new principle in the science of organization, in its reference to the kingdom of the invertebrata, reposes now on the firm basis of demonstration. The phenomena of gradation, rightly interpreted, has insensibly expanded into the *law* of organologic creation. If such a law be true of the invertebrata, is it conceivable that it can be untrue of the vertebrated series? The vertebrate organism begins at the limit at which the invertebrate ends. The architectural principles on which the former fabric is built are the direct and natural continuation of those observed in the case of the latter. The interval which removes the fish from the mammal is not the less real because it is less wide and conspicuous than that which divides the sponge from the cephalopod. In both examples the interval is traversed by a *graduated scale.* The principle of graduation stamps its impress upon the most subtle elements, and governs the deepest penetralia of the vertebrate as of the invertebrate organism.

The method of tracing upwards from the lowest type the history of *individual organs of the body,* gives to the law which regulates their increase in the line of the ascending genera a new force. Each organ has its own history. It has been ably written in the works of the anatomists of this age. There is, however, an unwritten history which remains to be added to the literature of organic science. A change in the outward form, the type, or the dimensions of an organ in the living animal body, cannot, from the introverted reciprocity which pervades all, occur as a solitary event. It must have been preceded by preparatory occurrences; it must have been succeeded by derivative sequences.

This chapter, when given to science, will enrich substantially its annals. Compare any single organ of the vertebrate animal with the corresponding organ, when present, of the invertebrate. The difference is remarkable. But compare the entire visceral solids of the one with those of the other. Look at them respectively as multipartite engines designed to accomplish a definite purpose. What disparity of power!

Now place in juxtaposition the products of the actions of these engines severally—the fluids of the invertebrate by the side of those of the vertebrated animal. Is the disparity less striking? It is scarcely possible to conceive that the *constructive idea*, in two things so widely dissimilar, can be the same. It is so. One is the prolongation of the other *plus* a few superadded elements.

The bright era will dawn upon the science of living beings when the physiologist, from the examination of the fluids, will be armed with knowledge to deduce the nature, the number, the dimensions, the structure of the productive viscera; and conversely to foretell the constitution and vital standard of the blood from a consideration of the viscera. In this communication it is only practicable to give to these views the form and shape of verbal propositions. The details of demonstrations are, for the present, postponed.

The blood of the fish differs neither less nor more from that of the cephalopod than the visceral solids of the two animals from one another. It cannot be otherwise, if the fluids owe their formation to the agency of the interior solids.

Excluding the lancelet, whose organs generally are inferior to those of myxinoid fishes, think of the *new elements* which at the fish are added to the machinery of the organism.

Though straight and short, the stomach and intestinal canal are capacious. It is here, for *the first time* in the zoological series, that the liver assumes a separate existence, and becomes internally complicated by a portal and arterial circulation, and a system of excretory ducts. Now the pancreas and the spleen first show their presence in the machinery of organization. It is only at this limit that the renal apparatus has acquired an evident importance. A new and hitherto unknown element of the animal body occurs also at this stage—*the absorbent system.* What *must* be the issue of these several complex acquisitions on the side of the elaborative organs, this skilfully contrived involution of parts and superimposition of specially created structures? The first and most unambiguous result is inscribed on the characters of the fluids. The proteinised principles are raised in amount, and probably improved in quality; colour (hæmatosine,) in a novel manner and place, has appeared. A higher class, a more finished order of floating corpuscles, enters on the scene. Is the ellipticity of their figure a character of degradation? But, withal, the heart still retains the impress of relative inferiority, and the *respiration is aquatic.*

In the *Reptilian organism*, what are the signals of elevation? The Batrachia exhibit a long and convoluted alimentary canal, but the stomach is only slightly individualized.

This part of the alimentary canal augments more and more, from the Batrachian, through the Ophidian and Saurian, up to the Chelonian orders. As the gastric segment dilates, the intestinal canal shortens. In reptiles, though the liver is relatively large, the product of its action is small, for it operates upon blood which is semi-arterialized. The size of an organ is frequently inversely as its texture; a large and loosely-structured organ does not really present so multiplied an operative superficies as the small but densely-textured. The *pancreas* betrays indications of higher com-

plexity, as measured by that of fishes.* The spleen rises to the consequence of a pronouncedly-developed organ; the heart acquires an additional auricle, and the first trace declares itself of the presence of a second ventricle. It is here that the grand transition is accomplished from aquatic to the atmospheric method of respiration, in the vertebrated series. Evidences of a more elevated standard are discernible in the fluids. The proportion of fibrin is undoubtedly augmented; the corpuscles are larger, more efficient as instruments for the generation of pigment; the specific gravity† progresses upwards. The entire apparatus of the exterior solids attests the thickening, advancing composition of the fluids.

In *birds*, the march of organological acquisitions proceeds. The whole digestive apparatus multiplies; its several constituent divisions are involved in the advance. To the œsophagus is appended a crop; after the crop follows a second dilatation, the proventriculus (the gizzard), the liver, the pancreas; to the system of the blood-making organs is added the spleen. The heart is perfected as a hydraulic mechanism; its contractions increase in frequency. The blood changes for the higher, in several respects; its fibrin more quickly coagulates; the corpuscles lessen in ellipticity of figure, and lose their nuclei. Respiration is quantitatively augmented. By comparison, the *sum* of the visceral machinery is multiplied unquestionably.

The *mammalia* crown the countless members of the consistent series thus cursorily and hastily sketched. Every individual organ here attains its maturity of structure; the arithmetic aggregate is greater, the component parts are more elaborately formed. The economic principle of the subdivision of labour is prosecuted to the utmost practicable extreme. On this far-up level stands *the human frame*, the perfection of animality.‡ It is here that the prerogative of the erect attitude is first conferred; it is here that a compactly-structured heart first drives the blood against gravity—a symbol of superiority. Discoidicity, in the figure of the mammalian blood-corpuscles, supplants the ellipticity of those of the lower vertebrata. In this class, the blood is more complex, of higher specific

* See the excellent article 'Pancreas,' by Dr Hyde Salter, Cyclop Anat. and Phys.

† In a future paper a tabulated series of observations will be given, by which the principle will be established, that the arithmetic sum expressive of the specific gravity of the fluids, conducted on the entire scale of the zoological series, will prove of unexpected value in elucidating the fact that the blood, viewed merely as a chemical solution, rises more and more in density as the scale is followed upwards from the lowest to the highest animal. Inferences deducible from such tables of facts will reflect corroboratively upon the reasoning, founded upon totally different evidences, which I have endeavoured to pursue in this and the preceding memoirs.

‡ The material fabric of the living body, animal and vegetable, is raised in standard by a progressive increase in the *number* and complexity of the parts or organs of which it is composed. The arguments pursued in this and the preceding memoir will, I trust, place this great theory of organization on a secure eminence. Can a wide-stretching law, which rules over the very elements of the ponderable organism, be untrue of its *imponderable moiety*? If so, there can be no constancy in Nature. But, in truth, the *phenomena* which express *a progressive ascension* from low to high, in what is trans-material, psychological, in living beings, are as unequivocal in clearness and significance as the signs which mark the advance of substantive individualities. The conclusion in the two cases is founded upon the same induction. The analogy is full and complete; it cannot be overthrown. Carry the eye downwards along that deep descending file of vitalities which connects humanity with the zoophyte. It may perform the journey by one of two separate yet dependent routes, the physical and metaphysical. As the organism rises in architectural finish, that immaterial principle, of which it is the tenement, rises in the same exact proportion. In the argument as developed in the text, it has been proved that product (the secretion) of the physiological activity is chemically complex if the producing organ be anatomically complex; incomplex if the latter be structurally simple. Why

gravity; the component viscera are weightier, relatively to the weight of the entire body. The mammalian characters culminate in man In this solitary *genus*, material and immaterial organizations reach the summit of refinement. On the subtlest and profoundest constituents of the collective system of interior productive solids is graven the impresses of the most finished elaboration; the middle system of the nutritive fluids are here, too, the theatre of the *most* complex chemistry. It is here, consequently, that the machinery of the exterior solids attains its most perfect and most beautiful development.*

The human hand, the faultless instrument of the human mind, the consummate symbol of *the completed scheme* of nervo-muscularity, *could not* be grafted on the organism of any inferior member of the series. That phase of bio-chemical science is fast approaching which will establish the prediction, that not only the visceral apparatus and the exterior solids, the system of the nerves and that of the senses, as compared with the corresponding elements in the bodies of all other animals, wear, indeed, the insignia of supremacy, but that the nutrimental media by which the "noble column," standing upright in express image of its Maker, is sustained, will yield to the micro-chemical philosopher of another day *proofs*, sublime in their wide-spreading and deep-penetrating significance, that *they also*, as integral parts of that privileged column, are clothed in the habiliments of *the same* supremacy.

should it, how could it be otherwise, if there be truth in demonstrative reasoning with reference to the *products* (psychological manifestations) of the activity of the nervous system, itself the sequence of a labyrinth of anterior activities? Could the human intellect sit enthroned on the contracted cerebrum of the fish? The question is ridiculous and incongruous. Could the liver of the radiated animal secrete bile, having the same chemical composition as that which is normally produced by the same organ in the cephalopod? The point is parallel and illustrative. In fact, violate by *any* supposition the principle of progressive seriality in the *psychology* of the animal chain, and the mind is landed in caricature and absurdity. The *human mind* itself is governed by the same law of progressive ascension. Unwind the tangle of this law in the history of nations, and the reality of its governance becomes undeniable!

* I trust that the *dependent physiological events* which, in the argument developed in this memoir, have been traced as a continuous line of light throughout the entire animal kingdom, will hereafter lead to the appreciation of *correlative dependent pathological events* in the study of the diseased conditions of the fluids The fluids are not physiologically isolated, how can they be so pathologically? If albumen and fibrin, &c, cannot arise and augment spontaneously, causelessly, it is highly probable that they cannot suffer disease primarily, causelessly, and spontaneously. Themselves being results, products, their morbid states are extremely likely to be also results or products of anterior disease. If on the entire scale of the animate series there be legible the bright characters of the all-pervading law—that *as the antecedent solids, so the consequent fluids, that as the antecedent fluids, so the second great system of solids*—it must follow that the same exact order must preside over the development of pathological phenomena. *Humoral pathology is a practical impossibility.* It is too partial and exclusive to embrace the truth. The *science* of pathology must rest on a wider basis. What does the *dogma* of humoral pathology really signify? That the fluids *per ipse* are pathologized! That effects can happen independently of causes! That causes can assume an operative form without ending in effects! I am deeply convinced that the modern revived phase of the humoral doctrine of disease is as partial and false as its archaic prototype. Exclusivism is the emblem at once of narrowness and ignorance. It is the obstructive bane of modern pathology. It ignores that inviolable reciprocity, that intimate neutrality of connexion, which federates into *one organism* the fluids and solids of the body. Morbidism in resultant fluids, it is impossible reasonably to conceive apart from disease in the causal solids. No single fact has as yet been recorded in pathology which proves that any one or more of the constituent principles of the blood may suffer disease *spontaneously*. If the antecedent processes executed by the blood-producing solids be normal, the result *must* be normal. Healthy fibrin, in its chemical place in the fluids, is under the sway of forces which, *if normal*, render the *dis*-order of its constituent atoms impossible; the disorder must be *communicated to it* down along a descending path of anterior changes; it is then diffused from it throughout the entire system of the exterior solids. No single advance can be made in the *science* of pathology but under the torchlight of discoveries achieved in the domains of physiology and organic chemistry.

Now reflect. A moral remains to be pointed. The tale is now to be adorned with one application of the principles of organogenesis, which it has been the hope of this paper to unfold. It is startling; first, from the awe-striking profundity of the region into which it conducts the human intellect; next, from the perfectly *natural and necessary manner* in which it logically flows from the successive and cumulative series of demonstrative propositions precedingly enounced. If it be permissible, even within the domain of *positive* philosophy, to rise, by fair reasoning, from the known to the unknown, from the peopled earth to a "plurality of peopled worlds," from the mortal to the immortal, from matter to spirit, from man's ways to God's, on the wide-opening wings of that imperious "Analogy" which enabled the divine genius of Butler to convince the sceptic, to open a path into the arid recesses of the mind of the most rigid mathematician, along which there *now* enters therein a luminous and beautiful "ray," whose far-stretching line spans athwart the nubial chasm which divides extant existence, conscious and demonstrable, from a bright and lovely futurity, *else indemonstrable* by the *ars instrumentalis* of human *reason*, the following analogical argument is also permissible.

In the most simple organism everything is simple; not the fluids only, but *all* the solids, productive and resultant. In the next organism above the lowest, *three classes of signs* mark an advance, though it be only *one step*, in its chemical and physiological characters. Ascendingly, when any given element or principle is *added* to the fluids, it may be predicated with entire certainty, that a correlative addition has been made to the productive solids, and that a proportionate advance *will result* in the exterior solids. Descendingly, reversely, *one* class of signs read exclusively would be utterly unintelligible; *all*, viewed in their connexion, open to the eye of the philosopher a *splendid prospect*. When Nature is about to add a liver, or a spleen, or a pancreas, or a cerebrum, to the system of the vital machinery already constructed, is it *her method* to perform the act causelessly, by the mystic interposition of a terror-striking, because wilful, capricious, *unsystematic* "Fiat?" Most reverently and most assuredly not. Her footsteps are most legible, most orderly, and, when accomplished, most logically *necessary*. The physiologist, in travelling upwards along the track of progressional creation, can *even now* foretell the appearance of a new element in the growing and involving machinery of the living body, with a confidence which is rivalled only by the axiomatic certainty with which the astronomer predicts the return of a comet, or the eclipse of a planet. *But let him travel downwards.* He successively leaves behind him organ after organ, principle after principle, element after element, until at length he arrives at the deep-down confines of *morphological organization*. Now rest for a new thought. Is the inquiring, restless, divine human intellect to *terminate* at this unexplanative, unsatisfying limit—a limit *at which God's creative processes are not limited?* What! are *rule* and *method*, are laws and processes, are constancy of direction and certainty of result, are logical sequence and necessariness of succession, which *everywhere else above this limit* are palpable in the creational workings of the Maker and Ruler of this illimitable universe, to be fooled, to be abandoned, to be reversed, to be contradicted at this particular, arbitrary, unmeaning confine, the reputed, but not the *real*, beginning of the atomic and elemental movements which

end, result, in the organized form of matter? *Credat Judæus!* It is as impossible as that to-morrow the earth will cease to revolve. Philosopher! adventure downwards still, but reverently, for thou art in the presence of the same Almighty whose footprints hitherto thou hast tracked with certainty and truth. Descend; cross the *unferried passage which divides organic from inorganic matter.* Art thou not on the same road? Most certainly. It is the clear and obvious continuation of that luminously-defined path by which thou hast descended from man to the "Amœbæa." Is thy eye to be here blindfolded? Art thou here to be bereft of reason? Why shouldst thou think, reason, and *observe* hitherto and no further? No! Be profoundly assured that thou hast the same just grounds for inquiring, analyzing, inferring, observing, &c. *below* this limit as above it. It is one of those legitimate spheres of study into which its All-knowing and All-seeing Author has *invited* the human mind. Be not afraid. "Elements and principles," inorganic, cosmic elements *move upwards*, combine and recombine, until, in the ascending march, organized *form* is attained in fearless and unswerving compliance with *those very laws and processes* by which they rise from the zoophyte to man. *Generatio equivoca!* The phrase is nothing but a hollow, senseless sound. Let it henceforth be put calmly aside. The march of thought, of inquiry, and of discovery will proceed. And the day, oh! glorious morn! *will* dawn on human science, when *the creation of species in time and space,* the appearance of a new being, a recently-constructed individuality, on the theatre of visible, palpable existence, will be as clearly, circumstantially, and minutely explained by human science, as it now defines the material conditions which lead to, render necessary, the *creation* of a new system of living parts, a new organ in the chain of serial organization. Spontaneous, equivocal generation! Let it henceforth be called *creation by rule, by conditions.* The act of *initiating* is the same as the act of *continuing.* The doctrine which contends for the *reasonableness* of the atomic, elemental movements, which result in the "spontaneous creation" of a new species in geologic time, is now the opprobrium of human science. The time will come when it will be its brightest and highest glory.

(*To be continued.*)

Art. II.

The Pathology of Insanity. By John Charles Bucknill, M.D. Lond., Physician to the Devon County Lunatic Asylum.

Whatever differences of opinion may be entertained respecting the causation of insanity when the excitant has been of a moral nature, the following propositions will scarcely be disputed.

Granting that the brain is either the organ or the instrument of mental power, and that physical causes are capable of producing insanity, in all cases so originating, an abnormal physical condition of the brain must be, and can only be, the cause of the abnormal condition of the mind.

To express this in more formal terms, we may say, that a physical agent acting upon a material substance can only produce a physical result. Blows, sun-strokes, poisons as of fever, are physical agents, which, acting on the brain, are capable of causing, and frequently do cause, insanity.

Insanity, therefore, in a considerable number of instances, is the expression of a physical condition of the brain. If the intimate physical state upon which it depends can be demonstrated, it will be for the metaphysicians to prove that all cases, howsoever produced, are not referable to similar conditions. The presence of a sufficient cause for certain phenomena having been ascertained, it is neither logical nor necessary to refer to the influence of other supposable causes. Physical causes are capable of producing syncope, moral causes also are capable of producing it; but since it has been proved by Dr. Burrows, in his work on the 'Cerebral Circulation,' that all physical causes operate by diminishing the pressure of blood in the cerebral vessels, it is fair to assume that all moral causes operate in exactly the same manner. To prove this assumption true, however, would not be easy; and theories about the moral causes of syncope, and the failure of spiritual essences, might be constructed as easily and rapidly as children make edifices of cards. The analogy holds good with regard to the physical and moral causation of insanity. If a physical cause can be demonstrated, it will be against all rules of scientific research to trouble and obscure the argument with other supposable causes, the very nature of which renders them undemonstrable. But is any peculiar condition of the brain, producible by physical agents, and causative of the phenomena of insanity, capable of proof?

We believe that it is. That it has not yet been demonstrated will readily be granted; but that it is demonstrable, we hold to be an opinion in conformity with the confirmed achievements of medical science in relation to other parts of the animal body. It is a subject which is knowable, and not unlikely to become known as soon as we have discovered the proper mode of questioning nature. It has frequently been objected, that because we are never likely to discover the final cause of thought, it is therefore not probable that we shall succeed in detecting the pathological cause of insanity. Thus, the author of the excellent article on 'Mental Diseases' in the 'Dictionnaire du Médecin Practicien,' states:

> "Le scalpel, le microscope, les réactifs pourront-ils nous montrer jamais le mécanisme des opérations de l'esprit? Sait-on pourquoi celui-ci est bon, celui-là méchant? Pourquoi l'un est spirituel, l'autre sot? Quelle est dont cette opiniâtreté a pénétrer les mystères de l'intelligence, lorsqu'on n'a jamais pu soulever le voile qui recouvre la vie organique? Jusqu'alors l'étude des formes et des manifestations nous a seule été permise. Il est probable que l'essence des choses, la cause première, nous echappera toujours."

Nothing, indeed, is more likely, for the *essence* of things, or final causes, are unknowable; whereas secondary causes are knowable, and are every day becoming known. If it were certain that the pathological conditions of insanity were rightly to be considered among the essences of things, well might we be disheartened at the prospect of fruitless labour in the attempt to unveil them. The essence of things has not been revealed to us, and is not discoverable by us. There is in nature a holy of holies, into which no high-priest of human faculties may hope to enter; are we therefore to desert the temple of science? Because we cannot reach the sun, are we to abjure the use of light?

Fortunately for the progress of medical knowledge, and for the welfare of the human race, the conditions of disease do not take rank among the

essences of things; they occupy a secondary and more accessible grade. We know as little of the essence of secretion as we know of the essence of thought: that is, we know nothing. We are utterly ignorant of the final cause why one set of cells separates from the blood, urine; why another separates bile, and so on. The essence of secretion, like the essence of thought, will in all likelihood be for ever hidden from us. But this reflection has not prevented the secondary phenomena of secretion from being discovered, and deviations from the normal conditions of these phenomena from being recognised as the conditions of disease. And is it not probable that we shall eventually discover the conditions of cerebro-mental disease, not in the inaccessible heaven of final causes, and dependent upon the essence of thought, but on the hill tops of natural phenomena, which are approachable by human patience and industry; not in the reveries of metaphysicians, but in the plodding pursuits of the pathological anatomist?

That investigations into the pathology of insanity have hitherto been somewhat unfruitful of results, need excite little surprise. Structural pathology, and its handmaiden, organic chemistry, have not long been cultivated with that diligence which has in many instances been rewarded with recent and splendid success. It is but yesterday that these sciences were in their nonage, and it would be absurd to object that they have not yet solved the most difficult problem which can be propounded. That diseases of the brain constitute the most difficult problem of structural pathology, there can be little doubt; and the principal reason for it may, perhaps, be found in the fact, that brain-substance bears no apparent relation to cerebral function. We observe a collection of minute cells and tubes, discharging certain functions, but of the most important of these, namely, the cells, we are as yet entirely ignorant of the origin, the development, and the decadence, of their arrangement, and their connexion with the tubes.

We doubt whether the most skilful microscopist, employing the most perfect instrument ever yet constructed, could recognise a single brain-cell, as such, distinguishing it from a cell of the same size taken from a gland. Yet we know it must have its capsule and its contents, its proper mode of development, its connexion by tissue or otherwise [its ependyma or stroma], with its fellows, with the capillaries from whence it derives nutriment, and with the nerve tube, from which it receives, or to which it communicates, impressions or influences. The cell may, at different periods, be dilated and contracted, like the corpuscular oxygen-carriers in blood. It may be filling itself, or growing from the capillary walls during sleep; and emptying itself, or decaying, during wakeful hours; it may lack its proper nourishment, owing to abnormal changes in the capillary walls, or of their fluid contents; it may contain its proper complement of phosphorized fats or other matters, or it may have become choked with cholesterine or other products of regressive metamorphosis, like the gorged fat-cells of the diseased liver or kidney; it may become drowned in serum, or entangled in condensed connective tissue. All these changes are possible, nay, even probable; but as yet we are unable to recognise any one of them.

The infinity of nature still defies the skill of man. Golden trophies

she drops for the ardent pursuer, but still she flees. The fact has been humorously expressed, as it relates to the minutiæ of zoology:

> "Each flea has its flea, with less fleas to bite him,
> And less fleas have lesser fleas *ad infinitum*."

It is not less true in relation to the minutiæ of pathology.

That the check which the microscopic pathologist has received before the neuroses will eventually cease, we entertain no doubt; although, hitherto, the labour expended in this field of science has been rewarded with a parsimonious hand. The main thing needed for the success, besides more powerful instruments and improved means of manipulation, seems to be, that the microscopic investigator should have under the eye, at the same time, both healthy and diseased brain. Appearances which can neither be remembered nor figured may be sufficiently evident as abnormal changes, if contrasted at the time with a healthy standard. A few granules more or less in each cell may make all the difference between health and disease; and this difference may be inappreciable, unless the cells in which it occurs are confronted, so that the observer may at the same time "Look here upon this picture, and on this." But is it necessary that investigations into the pathology of insanity should be postponed until the microscope is competent to reveal the minute alterations of nerve-cell and nerve-tube? The history of pathological research on other organs would indicate that it was not necessary. The changes wrought by disease in an organ have first been recognised, as it were, in the lump; the minute structural changes have revealed themselves to later investigation. Dr. Bright ascertained that dropsies with albuminous urine were accompanied by an altered state of the kidneys, easily recognisable in the mass, and without the aid of the microscope; and subsequently to his discovery, the minute structural pathology of these changes has been developed by several observers. Although the tendency of pathological discovery has been to development from the particular to the general, as relates to diseased conditions of the body at large; as relates to diseased conditions of individual organs; its development has taken place in the contrary direction—namely, from the general to the particular. May we not hope, therefore, that, as the coarse pathology of Bright's disease was recognised before the microscope had to any extent rendered its potent aid to the labours of the medical investigator; as the ruder facts of fatty heart were recognised by Laennec and others long before Dr. R. Quain's classical paper made this disease one of the best known in the range of medical science; in like manner, some coarse but important observations, relating to the pathology of cerebro-mental disease, may be established before the microscope brings its tardy help to elucidate the intricacies of cerebral organization?

It must be admitted that, for many years, the brain in the mass has been diligently questioned for the cause of insanity; and that the results, if not entirely negative, have been unsatisfactory. The brain of an insane person is very generally found to have undergone marked alterations, but these have not, apparently, been different from those found in persons who have died of other diseases without mental alienation. For instance: of two persons affected with the same amount of cerebral hæmorrhage, and apparently much in the same locality, one becomes paralysed, with some impair-

ment of mental powers perhaps, but without insanity; the other becomes insane. As yet, the pathologist is unable to trace the cause of this difference of symptoms. Esquirol, after having examined the brains of many hundreds of persons dying insane, was led to the conclusion, that no cerebral changes had been observed by him in such cases which were not also to be found in others where no insanity existed. The same observation has been made by other experienced pathologists, both in France and in this country; and it is impossible to look through any detailed list of post-mortems of the insane without feeling convinced that the special characters of cerebral changes have hitherto escaped observation; for that there are special changes, no exact reasoner can for one moment doubt.

Having, ten years ago, been placed in charge of an institution providing for the treatment of 450 insane patients, I have examined an average of 30 persons dying insane every year. During the first six years of this period, the only impression made on my mind by these examinations was, that Esquirol's observation had been founded on truth. I gradually, however, became aware of this leading fact—that the brains of all persons dying insane, except those of some epileptics, presented well-marked appearances of deficient or degraded nutrition: they were all more or less atrophied; and the majority of them were atrophied in a far greater degree than I had ever observed to occur in the brains of persons not dying insane.

Knowing that it would be useless to add to the existing accumulation of barren observations; and that nature having returned negative answers to inquiries put in one manner, it would be absurd to expect more positive ones unless the method of interrogation was varied; and the microscope having been employed diligently, but in vain; I bethought me of the most feasible plans for determining in what manner, and to what extent, the nutrition of the brain was altered. For this purpose, it appeared most desirable that the actual weight of the organ should be compared with that which would have been its weight in a state of health; and the indications of what this would have been were sought for in alterations of its specific gravity, and in the amount of shrinking from its bony case.

In the annual 'Report of the Devon Lunatic Asylum,' published at the end of the year 1851, I gave a list of examinations in which the capacity of the cranium, the weight of the brain, and the specific gravity of the cerebrum and the cerebellum, was marked. The specific gravity of the nervous centres has since been investigated by Dr. Sankey, Dr. Oliver, and others. Dr. Sankey's elaborate and interesting paper on this subject, published in this Journal in January, 1853, is particularly valuable, as affording means of comparison, his observations having been made upon persons not dying insane. I have myself published a second and a third series of observations in the 'Lancet' of December, 1852, and in the last two annual reports of this institution.

The question of specific gravity, however, although an important one, I have always felt to be subservient to that of atrophy of the brain, as indicated by shrinking of its substance, or by loss of specific gravity without shrinking. Variations in the specific gravity appeared likely to indicate that which the microscope would not show—namely, whether, as in degeneration in the nutrition of other organs, an excessive deposit of

albuminous material ever took place in the brain, rendering its specific gravity higher than usual; or whether, on the other hand, its specific gravity might not become lighter in consequence of fatty degeneration.

I think it highly probable that the rare and curious condition known as hypertrophy of the brain, may depend upon abundant interstitial deposit of albuminous material; and that the pathology of at least some cases of epilepsy may be owing to a less degree of the same condition. Careful organic analyses can alone solve this question with certainty; but in the meantime, the increased density and specific gravity of the brain in epileptic cases, and the analogy of similar pathological changes in other organs, render the opinion provisionally tenable.

The belief expressed in my paper above referred to, that many cases of diminished specific gravity are owing to the conversion of the proper brain substance into fatty matters, has of late received confirmation and authority from the researches of Henle and Meckel into the degeneration of the contents of the brain-cells into cholesterine, forming what they call violet-fat (*spek-violett*), from its blue reaction with iodine and sulphuric acid, and which Henle believes to be cholesterine arising from metamorphosis of the fat molecules of the nerve-cells.

In the paper above-mentioned, I stated that "I believed the investigations there recorded would establish the existence of two kinds of cerebral atrophy,—namely, positive atrophy, and interstitial or relative atrophy, which may or may not be coexistent. By positive atrophy I wish to indicate an actual shrinking of the brain, and by relative atrophy an interstitial change, wherein the active cerebral molecules suffer diminution, and inert materials are deposited."*

In the two first series of post-mortem examinations published by me, I endeavoured to show the amount of positive atrophy by comparing the external measurements of the cranium and its capacity with the weight of the brain. Professor Sharpey, however, suggested to me that the more simple and complete method would be to compare the capacity of the cranium for water, with the amount of water which the brain would displace. This plan I have since adopted, and have found it satisfactory. I have not, however, discarded the old plan of measuring the cranium, believing that an extensive series of accurate cranial measurements of insane persons will be interesting, and may eventually prove useful.

It will be needful to describe with some minuteness the method I have adopted, to ascertain the amount of positive atrophy, or the degree in which the brain has shrunk, in each instance, from the cranial parietes.

The brain, including the medulla oblongata, is slowly immersed in a vessel of convenient size and shape, which is filled with water up to the level of a capacious spout placed at an acute angle with the sides. Before the brain is so immersed, the contents of the ventricles, and any serum which may be in the sub-arachnoid tissue, are allowed to escape through several long incisions. The organ is not allowed to remain immersed long enough to imbibe water, which it is capable of doing in large quantity, as proved by the experiments of Nasse. As it descends in the vessel the water it displaces escapes from the spout, is caught and mea-

* Lancet, Dec. 25, 1852.

sured, and affords a criterion of the actual bulk of the brain. (See 18th column of Table.)

The capacity of the cranium* is obtained by a somewhat more troublesome process. It is well known that one of the older physiologists employed millet seeds for this purpose; Sir W. Hamilton used sand: but neither of these methods would be feasible in the recent subject. The plan I have adopted is as follows:—The foramina at the base of the brain are carefully plugged with tenacious clay—that used by statuaries for modelling answers best; a small triangular piece of the frontal bone is removed with the saw: the calvarium is readjusted to the base, the dura mater being left attached. The space left by the attrition of the saw in removing the calvarium is filled up with clay, and a narrow bandage, with clay spread upon it, is made to surround the cranium three or four times, covering this space. If this manipulation has been carefully done, the cavity of the cranium will now be found as tight as a bottle. Sixty fluid ounces of water having been measured, a sufficient quantity to fill the cranial cavity is now poured from it by means of a funnel through the orifice in the frontal bone, taking care that the stream does not wash away the luting of the foramina. The fluid which remains after having filled the cranial cavity, is measured, and being deducted from the sixty ounces, gives the amount employed. (See column 19 of Table.) Thus, if nine ounces and two drachms remain, the capacity of the cranium was fifty ounces and six drachms; and if the amount of fluid displaced by the brain was forty-five ounces, the amount of atrophy was five ounces and six drachms. To this must be added half an ounce occupied by the luting, giving the actual amount of atrophy as six ounces and two drachms. Of course this examination is made before the chest is opened.

It is proper to observe, that although the above process was satisfactory in the majority, there were a few cases [and those of the most interesting, on account of the extensive atrophy they presented] in which a difficulty was experienced in draining off the effused serum from the brain. In most instances the serum effused under the arachnoid readily flows away through incisions made for that purpose; but in some, where the effusion has existed for a long period, the sub-arachnoid cellular tissue having become hypertrophied and tough, the serum contained within its meshes cannot be entirely removed. To this cause must be attributed some of the discrepancies which may be discoverable in the accompanying table.

The brain of a healthy subject fills the cavity of the skull; so that the water it displaces, and that requisite to fill the cranial cavity, are very nearly equal. At the base of the cranium of a healthy adult, killed by

* Mr. Paget, in his Lectures on Surgical Pathology, vol. i. p. 78, says: "The hypertrophy of the skull, which may be called concentric, is that which attends atrophy with shrinking of the brain, in which there is diminution of its bulk. . . . . The thickening is attended by remodelling of the inner table," &c. . . . . "At whatever age, after complete closure of the cranial sutures, shrinking of the brain may happen, this hypertrophy of the skull may be its consequence." It is certain that this change is a very rare result of atrophy of the brain in the insane. It only occurred once in the 63 cases recorded in the table: and, in the course of my pathological experience among the insane, I have not met with it more than three or four times. Such skulls are not uncommon in museums, from which source, perhaps, Mr. Paget has derived his examples. An hypertrophied cranium is a preparation easily made, and which lasts for ever. There must be some other cause than atrophy of the brain to which the production of concentric cranial hypertrophy must be referred: perhaps often to venereal affection.

accident, it is rare to observe more than a few drachms of serum; and the sub-arachnoid space contains only enough serum to keep it moist.* It will be important to ascertain what amount of divergence from this standard may occur in old age, and in various wasting disorders without insanity. At the present time I have no opportunities for such observations. Where insanity has not existed long, and has not been intense, the brain is sometimes found to have shrunk in a degree scarcely appreciable.

In epileptic cases, where the disease has not produced any considerable amount of dementia, the brain is not found to have shrunken. The brains, indeed, of persons dying with epileptic mania are found to present no appearances of disease; the deviations from health which we are able to observe being attributable to the mode of death. In these cases our faulty and insufficient methods of investigation must bear the blame of failure.

In the 11 cases in the Table in which insanity was accompanied by epileptic seizures, the average amount of shrinking was only $3\frac{8}{11}$ ounces; the average of the whole number of cases being $5\frac{1}{4}$ ounces. In 6 of the 11 epileptic cases the amount of atrophy was inconsiderable, averaging less than two ounces; although, in several of these, the accompanying loss of mental power was great and of long duration. Cases of exaggerated dementia, in which, if uncomplicated, a large amount of cerebral atrophy might be confidently looked for after death, when of epileptic origin, often present a brain apparently well nourished and perfectly normal, or marked only by changes attributable to the mode of death. It must be acknowledged that the pathology of insanity caused by epilepsy is distinct from that of other forms, and that it can scarcely be said that a reasonable hypothesis has been as yet formed respecting it. In the examination of 33 brains of epileptics, I have only once found a spicula of bone projecting from the cranium, and once only a tumour: this pressed on the nodus encephali, and was mainly composed of plates of cholesterine.

The Table contains the measurements of 13 patients whose age exceeded sixty-five years, and in these the average amount of the cerebral atrophy was $8\frac{1}{4}$ ounces, or more than 50 per cent. above the average of the whole number. In 2 cases only was the atrophy less than three ounces, and in both of these cases the form of mental disease was mania, and the mental powers had suffered little loss.

The most common form of mental disease in old age is primary dementia, or an exaggerated condition of the state so well known under the name of second childhood. Cerebral atrophy is a constant concomitant of this form of mental decay, and may be always looked for, in

* Majendie entertains different opinions on this subject, and believes that a considerable amount of serum always exists in the sub-arachnoid space, serving the mechanical purpose of preventing concussion of the cerebral substance, as the liquor amnii preserves the fœtus. He describes four places, which he calls confluxes, where the cerebro-spinal fluid, as he calls it, collects in considerable quantities. The principal of these are formed by the reflexion of the arachnoid at the base of the brain. (See Récherches Physiologiques et Cliniques sur le Liquide Cephalo-Rachidien, 1842.) It is true that in perfectly healthy brains a small quantity of fluid, rarely exceeding half an ounce, is found at this spot, bathing the roots of the cerebral nerves. Majendie examined and measured the fluid from the spine and the brain together, obtaining it by tapping the lower end of the spinal theca. From observations I have made on the relative quantity of fluid to be obtained from the theca of the spine and cranial cavity, I am persuaded that the greater portion of the fluid in Majendie's experiments came from the former source.

extent varying with the loss of mental power which has occurred before death closes the scene.

Why, in some aged persons, the cerebral centres fail in the power of appropriating due nourishment at a comparatively early period, and while the digestive, the circulatory, and the respiratory systems are still healthy and vigorous, may be owing to hereditary predisposition, or to their having been subjected to more work, or to ruder shocks of emotion, than common.

That cerebral atrophy in aged persons is not dependent upon failure of the functions of alimentation and general assimilation, is shown by the fact, that persons suffering from such atrophy are for the most part well nourished, as relates to the body at large. That some cases of cerebral atrophy do depend upon defective alimentation is more than probable. When persons have been starved to death, cerebral symptoms allied to mania have never been wanting. Whether these have been owing to the influence of decayed matter not eliminated from the brain, in consequence of new material not being supplied; or, as Liebig thinks, in consequence of commencing oxygenation, or eremacausis of the cerebral molecules,—they prove that the encephalon suffers speedily and seriously from want of a due supply of nutritive material.

But it may be asked, in reference to senile dementia, whether a gradual decay of the great nervous centre is not an inseparable concomitant of advancing age, and whether the failure of its functions is not the certain and necessary cause of death, in default of other causes which may all be called accidental? When the golden bowl of life is not broken by chance, is not the nervous tissue the silver thread which must give way under the tension of age and the implacable shears of destiny? The constant decadence of mental power in advancing life, and the annihilation of mind where the course of life has been greatly prolonged, indicate that such is the fact. The repute for wisdom possessed by age is perhaps mainly due to the decay of the passions being more rapid than that of the intelligence. If a man does happen to pass into the second century of his existence, sound in wind and limb, heart-whole and hungry, his mind never fails to give way. The Nestors of history are more than mythical,—they are impossible. And the proverb, that "the strong man dies upwards," is untrue; the strong man, like the aged oak, decays first at the top. The period when the decay commences is very variable. Southey relates a good story of a traveller arrested by the pitiful sight of an aged man sitting in the porch of a farm-house and weeping bitterly: in reply to an inquiry as to the cause of his tears, the old man said he wept because his father had beaten him. Curious to ascertain the truth of so strange an assertion, the traveller entered the house, and encountered a still older man, but hale and vigorous. He acknowledged that he had beaten his son, and that it served the latter right, for he had been plaguing his grandfather,—pointing to a mindless centenarian in the chimney corner. The grandson was in his dotage, the grandfather was mindless, but in the middle term of this strange family group mental decay had scarcely commenced.

The greatest amount, however, of cerebral atrophy observed in any case, was unconnected with advanced age. The case was so remarkable and instructive that I shall venture to give a few of its details.

EXAMINATION OF THE ENCEPHALIC ORGANS IN SIXTY-THREE CASES OF INSANITY.

Patient's number.	Sex.	Age.	Form of mental disease.	Supposed cause of mental disease.	Duration of mental disease.	Apparent cause of death.	Measurements of body. Height.	Measurements of body. Breadth from one acromion to other.	Measurements of cranium (scalp removed). Circumference.	Measurements of cranium. Between meatus and over vertex.	Measurements of cranium. Root of nose to occipit protuberance.	Thickness of grey matter of cerebral convolutions in 100ths of an inch.	Specific gravity of cerebrum: tubular neurine.	Specific gravity of cerebrum: vesicular neurine.	Specific gravity of cerebellum.	Weight of brain and medulla oblongata (avoirdupois).	Displacement of water by brain in fluid ounces (apothecaries).	Capacity of cranial cavity for water (fluid ounces, apothecaries); half an ounce added for luting.	Amount of atrophy shown by difference between displacement and capacity.
							ft. in.	in.	ft. in.	in.	in.	in.				lb. oz.	oz.	oz.	oz.
1196	M.	28	Dementia	Blow on head	3 years	Apoplexy, with convulsions	5 3½	15	1 9½	13½	14	7/100	1·034	1·040	1·045	3 0¼	44¾	48	3¼
155	F.	37	Idiotcy	Malformation	Congenital	Heart disease	4 7	13	1 8	13	13¾	7/100	1·042	1·034	1·042	2 0¼	37¾	40½	2¾
809	M.	18	Epilepsy	Epilepsy	4¼ years	Peritonitis	5 9	14	1 8¾	12¼	14⅓	8/100	1·034	1·041	1·041	3 3¼	40	49½	¾
1121	F.	23	Melancholia	Unknown	11¼ months	Phthisis	5 4	13½	1 8	12	13⅗	8/100	1·041	1·034	1·041	2 8¼	39	41½	2¼
523	F.	77	Mania	Unknown	7 years	Phthisis	5 1½	14	1 8¾	12	13	7/100	1·040	1·048	1·035	2 13	43¼	45½	2¼
1186	F.	67	Chronic mania, with dementia	Death of husband and son	5 yrs. 9 wks.	Old age	4 10½	13	1 8	11½	12	7/100	1·032	1·032	1·041	2 8¾	37¼	43½	6
1133	F.	30	Mania: general paralysis	Reduced circumstances	11 months	General paralysis	5 3½	12	1 8½	12½	12⅔	7/100	1·034	1·034	1·040	2 9½	40¼	47½	7
377	F.	33	Idiotcy	Malformation	From birth	Apoplexy	4 8	11¾	1 7	11⅓	12¾	7/100	1·043	1·038	1·039	2 7¼	36¾	40½	4
498	F.	41	Epileptic mania	Puberty	Unknown	Epilepsy	5 2½	15	1 8½	12¾	13	7/100	1·041	1·040	1·041	3 1¼	45¼	51½	6¼
255	M.	57	General paralysis	Blow on head	Unknown	Convulsions	5 6½	15	1 10	13¼	14	9/100	1·041	1·041	1·041	2 13¾	43½	52	8½
147	F.	53	Mania	Disappointed affections	Unknown	Bronchitis: phthisis	..	12½	1 8	13	13⅞	7/100	1·035	1·035	1·035	2 11¾	41	44½	3½
1129	M.	66	Dementia	Fever	4 yrs. 8 mos.	Softening of brain	5 6½	14½	1 9½	13	14⅓	7/100	1·036	1·032	1·032	2 14¼	43	56¾	13¾
1127	M.	21	Epileptic mania	Too rapid growth	4 years	Epilepsy	5 5	14	1 8	11	13¾	9/100	1·042	1·041	1·042	2 0¼	42	44	2
1181	F.	38	General paralysis	Dissolute habits	Unknown	Paralysis with epilepsy	5 5½	13	1 7½	11¾	12⅞	7/100	1·042	1·041	1·042	2 9	40	42½	2½
1067	M.	24	Melancholia: deaf and blind	Reduced circumstances	1 year 7 mos.	Phthisis	5 7	15	1 8½	12	12¾	7/100	1·044	1·035	1·049	2 14¾	44½	52½	8
1064	F.	60	Melancholia	Unknown	2½ years	Paralysis	5 2	13¾	1 8	12	13	7/100	1·036	1·033	1·042	2 11	39	40½	1½
1197	M.	27	Idiotcy	Malformation	From birth	Phthisis	5 5	13	1 9	13	13¾	8/100	1·040	1·038	1·042	2 15½	40	44½	4½
869	M.	37	Mania	Unknown	5 yrs. 3 mos.	Results of apoplexy	...	13	1 9	12⅔	14	7/100	1·033	1·035	1·034	2 7¼	37½	52½	15
1223	F.	55	Mania	Prostitution, intemperance	5 months	Results of apoplexy	5 7	13½	1 8	13	13¾	9/100	1·041	1·035	1·041	2 7	37	42	5
517	M.	35	Epilepsy	Epilepsy	2 months	Phthisis	5 8	15½	1 8½	12	13	7/100	1·041	1·039	1·040	2 13¼	41¼	48	6¾
1040	F.	75	Chronic mania	Unknown	2 months	Bronchitis	4 10	11½	1 8¾	12	12⅓	7/100	1·039	1·039	1·041	2 12	41	47½	6½
431	F.	13	Epilepsy	Fright	7 yrs. 5 mos.	Bronchitis and epilepsy	4 3	10	1 8	12¾	11⅓	7/100	1·039	1·039	1·039	2 12	42	44	2
712	M	19	Melancholia	Anxiety abt. business	4 yrs. 1 mo.	Dysentery and heart disease	5 6½	15	1 8½	12	13⅓	8/100	1·036	1·038	1·036	2 13½	44	44	0
488	M.	72	Dementia	Not known	Congenital	Chronic pleurisy	6 6	14	1 10	13	14⅛	8/100	1·036	1·036	1·036	3 7	53	58½	5½
210	M	24	Epilepsy	Not known	Unknown	Epilepsy	5 4½	14	1 9½	12⅓	14¼	7/100	1·049	1·041	1·044	3 11	56	58½	2½
763	F.	40	Mania	Intemperance	4 yrs. 4 mos.	Phthisis	5 7	14½	1 8½	11½	13	7/100	1·041	1·037	1·041	2 11	42½	43½	1
701	M.	68	Mania	Not known	7 years	General paralysis	5 7	16	1 9½	13¼	13¾	8/100	1·042	1·033	1·042	4 0	46¾	55½	8¾
749	F.	90	Mania	Reduced circumstances	4 years	Old age	5 5	12	1 9¼	12	14	8/100	1·037	1·041	1·042	2 12½	40	48	8
849	F.	31	Epileptic dementia	Epilepsy	17 years	Epilepsy	5 6	14	1 9½	12½	13	8/100	1·038	1·038	1·038	3 3	47½	50½	3
264	F.	63	Incoherence	Not known	Unknown	Inflammation of the lungs	5 2½	13½	1 8¼	11	13	8/100	1·040	1·037	1·043	2 13½	40	43	3
972	M.	46	General paralysis	Not known	2 yrs. 3 mos	General paralysis	5 3	13¼	1 9	12½	13⅓	[illegible]/100	1·032	1·033	1·042	2 13	40	50¾	10¾
1028	F.	51	Recurrent mania	Not known	3 yrs. 5 mos.	Syncope: heart disease	4 10	13½	1 8½	13½	13	7/100	1·043	1·036	1·046	3 1½	46¼	52	5½
1163	M.	38	Mania	Hereditary predisposition	4 years	Pneumonia	5 8	15	1 8¾	12	13¾	7/100	1·042	1·035	1·041	2 14	45	49⅓	4½
738	M.	48	Epilepsy	Unknown	Many years	Epilepsy: heart disease	5 3	15	1 8	11¼	11¼	8/100	1·039	1·035	1·042	3 12	41½	49	7½
673	M.	45	General paralysis	Fright from shipwreck	5 yrs. 3 mos.	General paralysis	5 4	15	1 0¾	12¾	12	[illegible]/100	1·040	1·047	1·042	2 8	40	44½	4½
569	F.	41	General paralysis	Unknown	7 years	General paralysis	5 0	13½	1 8	11⅓	13	8/100	1·040	1·038	1·045	2 10½	41¾	42¼	¾
1151	F.	33	Mania	Dissipation	5 weeks	General paralysis	4 6½	14	1 7½	11	11½	7/100	1·040	1·036	1·040	2 7	38	40½	2½
905	F.	20	Epileptic mania	Fever	7½ years	Phthisis	5 0	13	1 8½	11¾	12¼	7/100	1·041	1·038	1·041	2 14¼	46	47½	1½
1138	M.	74	Mania	Unknown	6 months	Dysentery: diarrhœa	5 2	14	1 7¼	12½	12	7/100	1·032	1·030	1·030	2 11½	41¾	44½	2¾
1068	M.	31	General paralysis	Intemperance	1½ year	Dysentery: diarrhœa	5 1	13	1 8¼	12	12⅓	7/100	1·043	1·043	1·045	2 10	47	50½	3½
1083	M.	72	Dementia	Erysipelas of head	4 yrs. 7 mos.	Diarrhœa	5 4½	14	1 9½	12	12⅛	9/100	1·040	1·040	1·040	2 10	41	55½	14½
866	M.	34	Recurrent mania	Irregular living	2⅔ years	General paralysis	5 7	14½	1 9	12¾	12½	7/100	1·040	1·039	1·040	2 15	47	50½	3½
898	M.	37	Mania	Unknown	2¼ years	Pleuritis	5 6	15½	1 8	11¾	11¾	7/100	1·040	1·035	1·041	2 15½	45½	46	½
945	M.	65	Chronic mania	Unknown	Unknown	Diarrhœa	5 8	15	1 9	12	13	7/100	1·041	1·032	1·041	3 1	48	53¼	5½
614	F.	64	Melancholia	Hereditary predisposition	24 years	Pleuro-pneumonia	4 10	12	1 7½	12	11¼	8/100	1·038	1·038	1·038	2 8¼	40	44½	4½
952	M.	41	General paralysis	Destitution	2 yrs. 1 mo.	Apoplexy	5 6	12	1 8¼	12	12	7/100	1·041	1·037	1·041	3 12	43¾	[illegible]	5¾
1110	M.	40	Mania	Disappointed affection	Unknown	Apoplexy	5 6	15	1 8½	12½	13½	8/100	1·037	1·033	1·038	2 10½	43	52	9
438	M.	31	Dementia: epilepsy	Fright	12 years	Phthisis	5 5	14	1 9½	12½	13½	7/100	1·039	1·033	1·037	3 3	50	54¼	4½
1100	M.	50	Melancholia	Intemperance	8 months	Phthisis	5 9	15	1 10	12	13	9/100	1·040	1·032	1·042	3 0¼	55	57	2
1106	M.	52	Dementia	Unknown	13 months	General decay	5 9½	16	1 8	11½	13	7/100	1·041	1·040	1·046	2 10¾	42¾	44½	1¾
595	F.	78	Dementia	Unknown	3 years	Gradual decay: diarrhœa	5 4½	14	1 9¾	12	13	7/100	1·040	1·038	1·040	2 11	43	52	9
1099	F.	68	Mania	Unknown	6 weeks	Gradual decay	4 8	12	1 8½	12	12⅛	7/100	1·040	1·036	1·043	2 5¼	37	47½	10½
1018	M.	61	Melancholia	Intemperance	1 year	Heart disease	5 8	15½	1 9½	13	13½	7/100	1·041	1·036	1·041	3 1	47	55¼	8¼
643	F.	29	Idiotcy	Unknown	From infancy	Chronic peritonitis	5 1	12⅛	1 7¼	11¾	12¼	7/100	1·040	1·038	1·040	2 7¾	40	43	3
644	M.	72	Mania	Unknown	7 years	Heart disease	5 4½	14	1 8½	12½	12½	8/100	1·046	1·040	1·046	2 8¾	38	46½	8½
776	F.	78	Dementia	Unknown	3 years	Gradual decay: diarrhœa	5 4½	14	1 9¾	12	13	7/100	1·040	1·038	1·040	2 11	43	52	9
1894	F.	12	General paralysis	Prostitution	9 months	Apoplexy	5 2½	14½	1 7¾	11¼	11¾	7/100	1·041	1·039	1·044	2 10½	42	43½	1½
1245	F.	38	Mania, chronic	Unknown	2 years	Phthisis	5 0	13	1 8	13	13⅛	8/100	1·041	1·030	1·041	2 8½	40	45½	5½
1125	F.	49	Mania, chronic	Death of husband	7 years	Gradual decay	5 3	14	1 8	12¼	13	8/100	1·034	1·034	1·034	3 0	48	50½	2½
1158	M.	43	General palsy	Blow on head	1½ year	Paralysis	5 9	15	1 8½	12½	13⅜	7/100	1·037	1·031	1·037	2 11¾	41¼	50¼	9¼
1262	M.	70	Dementia	Unknown	Unknown	Disease of heart and lungs	5 5	15	1 9¾	13½	13	7/100	1·040	1·030	1·040	2 12½	43	53½	10¼
345	F.	40	Epilepsy	Unknown	Unknown	Epilepsy	4 10	11½	1 7¾	13¼	11½	7/100	1·044	1·044	1·044	2 9¾	41	45¼	4½
1150	M.	52	General palsy	Unknown	2 years	Gangrene of lung	5 6	15	1 10	13¾	13⅜	8/100; 6/10[illegible]	1·040	1·040	1·040	3 2	49¼	59¾	10½

phalon?—a condition which may pass into that of health on the one hand, or, on the other, into that of chronic degradation and atrophy? All the causes, even of the acute forms of insanity, point to an interference with the due nutrition of the brain. Starvation causes raving delirium; mortification produces muttering delirium; the poison of typhus, which, perhaps, more than any other agent, interferes with the nutrition of all parts of the body, throws the mind into a state between melancholy and stupidity, and not unfrequently causes maniacal excitement. It is highly probable also that even the moral causes of the acute forms of insanity do not operate in a different fashion. If anything is positively known of the brain and its functions, it is that it expends its powers during the waking state; and that it is nourished, and its powers are recruited, by cell-growth or otherwise during sleep. But the moral causes of insanity are especially those which "murder sleep," and thus the conditions of due cerebral nutrition are prevented. During the prodromic period of threatened insanity, opiates often act like a charm. Acute madness from moral causes rarely, if ever, comes on without preceding sleeplessness, that is, without a state in which the cerebral structure is for a long time undergoing waste without repair. After insanity has become established, the power of sleep is often regained without benefit: the balance of cerebral exhaustion and nutrition having been upset, is not always so easily righted; the sleep gained being only sufficient to repair the last losses of the organ, and not those which have been accumulating; it may be able to meet current expenses, but not to pay off old debts. And this may account for the fact, that chronic lunatics are not, on the whole, bad sleepers.

But although the production of sleep does not ensure the recovery of a patient labouring under acute mania, with as much certainty as it does that of a patient with *delirium tremens,* it is nevertheless true that the most satisfactory and rapid recoveries from acute mania take place in consequence of the early and powerful influence of "nature's sweet restorer." There is no better guide in the prognosis of a recent case of acute mania than the occurrence of deep, long, and refreshing sleep, or the persistence of restless insomnolence. The one gives the brain a chance of regaining an equilibrium between nutrition and excrementation; the other plunges it down the hopeless descent of regressive metamorphosis.

The progress of such deterioration may be rapid or slow. It would, at the present time, perhaps, be rash to venture an opinion how soon it may be measured. It appears to me highly probable that many instances of cerebral disease which have been designated by the ambiguous term of serous-apoplexy, are, in reality, owing to interrupted nutrition of the cerebral substance and rapidly proceeding atrophy. We have seen that, if the decay of the nervous functions takes place slowly, the life of an individual may be prolonged for a considerable time after sensation, voluntary motion, and cerebral power have been almost annihilated. But when the interference with cerebral nutrition has been more sudden, the checked innervation of the body is accompanied by more acute symptoms, greatly resembling certain cases of apoplexy.

No. 1125, a woman, aged 49, in consequence of grief at the death of her husband, suffered during six years from occasional maniacal excite-

ment of mild character; in July, 1853, she suddenly became paralytic over the whole body; the loss of power over the lower extremities was most conspicuous, but all the voluntary and the semi-voluntary muscles appeared to participate. She had great difficulty in swallowing, and her speech was unintelligible. The paralysis did not preponderate on either side of the body. She slowly recovered sufficiently to move about without help, and to articulate with distinctness. In the second week of August of the present year the symptoms returned, and in a few days she became completely paralytic over the whole body. The paralysis affected the abdominal muscles, the bladder, the orbicularis palpebrarum, the muscles of the pharynx, and those of the tongue. The tongue lolled out of the mouth; even the muscles of respiration were partially affected, the breathing being slow and shallow. For several days the pulse was good; it then failed. A careful examination of the body (which was well nourished) showed all the organs to be healthy, except the cerebro-spinal centres, which were much atrophied.

In the wards of this asylum I have seen many similar cases, and a still greater number wherein the symptoms of nervous decay have been less rapid in their progress, and less unmixed with those of disordered action of other organs. The loss of innervation from atrophy of the brain shows itself more frequently by causing some other weak organ to give way, than by purely nervous symptoms. Either a diseased heart, with which, however, a man with healthy brain might have lived many years, ceases to beat, from failure of the nervous function; or lungs or extremities become gangrenous; or inflammations of low type establish themselves. The immediate cause of death, in fact, may be found in various parts of the body, the remote but primary cause being impaired or debilitated nervous function.

The rapidity with which cerebral atrophy marches towards a fatal termination, is rarely so great as in the last instance cited, or so slow as in the former one. The deaths which, in asylum obituaries, are to be found attributed to such causes as "gradual decay," "failure of powers of nature," "decay of nature," are, we believe, mostly due to cerebro-spinal atrophy, and its train of symptoms.

Sir Henry Holland, in writing on what he calls deficiency or defective production of nervous power, says—

> "I find in my notes two or three singular cases, in which there existed what could only be interpreted as a deficiency of nervous power, without any obvious bodily disorder, except what this deficiency produced, and unconnected with any aberration of mind, but testified by a general torpor of all the functions of both. In the most remarkable of these cases, where the symptoms coming gradually upon a vigorous frame of body, lasted for months, all the voluntary movements of walking, speaking, eating, &c. were in a sort of abeyance—the mind inert, as if unable to force itself into any effort of thought or feeling, the circulation very feeble, and great torpor of the natural functions. The cessation of this state was as gradual as its commencement, and as little explained by any obvious cause."*

These cases strongly resemble those I have referred to, and were probably owing to a temporary and removable impediment to the due nutrition of the cerebro-spinal axis, occurring in persons with a healthy assortment

* Chapters on Mental Physiology, p. 279, note.

of other viscera. Those met with in asylums arise from a more grave and deeply-seated deficiency in the nutrition of these all-important organs.

With its full composition of causes we are at present unacquainted. At the pathological origin of atrophy of the brain, it is, at present, difficult even to guess. There are many reasons for believing that it is unconnected with constitutional tuberculosis. The insane, as a class, are not more liable to phthisis than the sane, perhaps less so. In Sir A. Morrison's paper 'On the Statistics of Insanity,' he shows that during thirty years he has treated 6779 cases of insanity in different public institutions; that of these 1440 died, of whom only 164 died of pulmonary consumption. Tubercle is very rarely deposited in the brain after childhood. I have myself only found tubercle in the brain in one solitary instance, this being an idiot boy, who died of tubercular peritonitis. Again, insane patients who die of phthisis, do not present a greater amount of cerebral atrophy than might have been anticipated from, or that corresponds with, the amount of mental decay. Of the 11 patients in the Table who died of phthisis, the average cerebral atrophy was $3\frac{3}{4}$ ounces, or $1\frac{1}{2}$ ounce below the average of the whole number. In the few phthisical cases I have examined, in which the mental symptoms have been recent and moderate in degree, the brain was found to have undergone only a slight degree of atrophy. No. 763 in the Table presents a good example. She was four years four months insane, but retained much mental power. The cerebral atrophy was only 1 ounce. As far, therefore, as the individual is concerned, there appears to be no connexion between tuberculosis and cerebral atrophy. But if the question is somewhat widened, the answer may be different, since there can be little doubt, that persons belonging to consumptive families are more liable to insanity than others. An hereditary and radical fault of nutrition, which, in some individuals of certain families, develops itself in the form of pulmonary consumption, produces in others a tendency to the cerebral changes which we are discussing. Also the modes of life which in some produce phthisis, in others produce insanity, dependent upon faulty nutrition of the brain substance; and I feel myself justified in making the assertion, that the hygienic, if not also the therapeutical, measures which are most useful in preventing or relieving tuberculosis, are the most efficient in the prevention or the treatment of insanity. A young lady of weakly constitution and sensitive mind is compelled to battle for a livelihood as a governess, and if distressed by adverse circumstances, the chances are great, that if she does not become consumptive, she will become insane. An artizan, condemned by the exigencies of a large family to work hard, to fare badly, and to breathe the tainted air of the crowded workshop, or the more crowded lodging, is exposed to the same alternative; the probability being, that if the brain is more excitable or impressible than common, the radical fault of nutrition will manifest itself in that organ; but if otherwise, that the lungs will give way. In either case, an early change of circumstances, with country air, cheerful occupation, nutritious food, tonic remedies, as the shower-bath, and, above all, sufficient rest, are often found capable of checking the progress of symptoms, whether they be cerebral or pulmonary, and not unfrequently of effecting a cure. These considerations col-

lectively are more than enough to establish a strong analogy between the most common pathological conditions of insanity, and of that peculiar disease of the system (tuberculosis) which no one doubts to be dependent upon perverted nutrition.

Cases of insanity undoubtedly exist which are, in the first instance, attributable to causes acting by the production of inflammation of the brain, or its membranous envelopes; but these exceptions confirm rather than invalidate the rule, insanity being in them secondary to, and consecutive upon, the inflammatory symptoms, and dependent upon the nutrition of the brain being interrupted or vitiated by the inflammatory changes. The analogy we have before used will again serve to explain our meaning; since it is universally admitted that inflammatory diseases of the lungs are frequently succeeded by the development of tubercular deposit. Bronchitis and pneumonia are attended by groups of symptoms, distinguishable, but not altogether dissimilar to those of phthisis; so also the symptoms of phrenitis are distinguishable, but not altogether unlike those of insanity. The means of diagnosis are far more complete in the one instance than in the other; but at the present day, the pathologist who is unable to distinguish between phrenitis and mania will scarcely be thought a first-rate craftsman. That a purely inflammatory condition of the brain may be one stage of insanity, is as true as that an inflammatory condition of the lungs may be the earliest stage of pulmonary consumption—that is, strictly speaking, it is not true; but the inflammatory condition may be, and frequently is, a preceding but distinct disease, to which true insanity is consecutive. That inflammation is more frequently followed by defective nutrition in the brain than in the lungs, is no doubt owing to the latter organs having a far greater power of repairing pathological injuries than the former. Pericarditis is a frequent cause of fatty degeneration of the heart; and it must be considered an established rule, that a common source of perverted nutrition is the change effected in the intimate structure of organs by inflammatory action.

Opinions on the ultimate nature of the nutritive defect which results in cerebral atrophy and insanity, must necessarily be speculative, since the ultimate nature of nutrition itself is unknown to us.

Its apparent and exciting causes may be classified as follows:

1. In predisposed persons it may depend upon poverty of blood, since it is producible by deficient food and by diseases interfering with the alimentative processes; and since an analogous train of symptoms occurs during starvation.

2. It is probable that in other cases it may depend upon some derangement of "the intimate connexion between the nervous and vascular systems, through which their most important functions are performed." Because it is sometimes found to be accompanied by extensive disease of the minute cerebral capillaries, the coats of which can be shown to be subject to fatty or earthy decay.

3. A third class of cases would appear to be producible by the molecular change effected by blows or violent concussions, and followed by atrophy, owing to some process as yet unknown to us. Atrophy of a testicle from a blow, without inflammation, presents an analogous instance.

4. Another class of cases are those following inflammation, and perhaps also following frequent or long-continued congestion. The basis of inflammatory action is an abnormal state in the mutual relationship between the blood and the tissues. That this state effects changes in the tissues, which, if not speedily repaired, must be followed by conditions of degraded nutrition, is proved by the pathology of every organ in the body. The brain certainly offers no exception. The capillaries become blocked-up, or their coats become spoiled for the purposes of nutritive regeneration of the tissue.

It also appears probable that, during inflammatory or congestive conditions, albuminous matter or serous fluid may be effused by the capillary network into the intimate structure of the brain; thus separating its vesicles and tubules from the capillaries, and preventing the due nutrition of the elements of nerve-structure. For this form of atrophy, we have formerly suggested the prefix of *relative*, as it may exist where there is no shrinking of the brain; atrophy with shrinking being termed *positive*. The two, however, may, and frequently do, co-exist.

5. The most numerous class, however, is that which depends upon want of rest, and the especial period of nutrition of the brain—namely, sleep. Want of refreshing sleep I believe to be the true origin of insanity, dependent upon moral causes. Very frequently, when strong emotion tends to the production of insanity, it causes, in the first instance, complete loss of sleep. In many cases, however, the power of sleeping is not lost, but the quality, so to say, of the function is perverted, the sleep being so distracted by agonizing dreams that the patient awakens jaded rather than refreshed. I have known several instances in which patients becoming convalescent from attacks of acute mania, have distinctly and positively referred to frightful dreams as the cause of their malady; and it is probable that a certain quality of sleep, in which dreams exciting terror and other depressing emotions more forcibly than waking events are likely to do, is not less adverse than complete insomnia to the nutritive regeneration of that portion of the brain on whose action the emotions depend. In such a condition, it is highly probable that the very portions of the brain which most need a state of rest are even, during the sleeping quiescence of other portions, more wastefully engaged in the activity of their functions than they could be in the waking state. The mainspring of insanity is emotion of all kinds; this, stimulated by phantasy, and emancipated from the control of judgment, during harassed sleep, may be more profoundly moved than at any other time. Bichat considered sleep to be a very complex state, in which it was possible for the cerebral functions to be in very different conditions of quiescence or activity: "Le sommeil général est l'ensemble des sommeils particuliers;" and he considered that dreams represent the active or waking condition of certain of these functions during the repose of the others. In this manner, a patient, some one or other of whose emotions has been profoundly affected, may continue to be sleepless, as far as the activity of the particular emotion is concerned, although he may by no means be the subject of general insomnolence; and this consideration will afford what seems to be a fair explanation of the exceptional cases to the rule, that the moral causes of insanity act by preventing the due nutrition of the brain, as it occurs during sleep:

"His slumbers—if he slumber—are not sleep,
But a continuance of enduring thought."

The only cases of insanity which are not thus reconcilable to this rule, are those which arise from the gradual development of one mental faculty, by frequent exercise, until its power becomes preponderating and excessive. Such cases may and do arise without loss or deterioration of sleep. But are they rightly classed as cases of disease of the mind? They seem to be instances of irregular development of the mental faculties, but not of morbid change; and, in the opinion of the writer, they cannot truly be considered instances of disease, a term which implies morbid change.

Delirium tremens presents an instance of real insanity (*plus*, an affection of the spinal nervous system), in which the intimate relationship between the symptoms of mania and those of insomnia are remarkably apparent; and cases of unmixed mania are numerous in which this relationship is not less intimate. Mania follows sleeplessness; if sleeplessness is obviated by appropriate remedies while mania is threatening, the explosion of the latter is prevented; and even after the invasion of maniacal symptoms, if continued and refreshing sleep is procured, these symptoms are frequently cut short. The relationship is unquestionably that of cause and effect. In the clinique of hospitals for the insane, cases do undoubtedly occur in which the cerebral symptoms are exaggerated after sleep; but the sleep in these cases is not continued and refreshing; it is broken sleep, sufficient only to recruit the sensory-motor energies of the patient; sleep in which the portions of the brain implicated by disease do not participate. Such cases are often, also, of a mixed character, between mania and phrenitis, in which the cerebral congestion produced by sleep has a prejudicial influence upon those portions of the brain tending to an inflammatory condition: for it must not be forgotten that the brain is a large and complicated viscus, and that different portions of it may at the same time be in very different pathological states. Congestion, inflammation, regeneration, nutrition, or degenerative atrophy, may all co-exist in the brain, as in the lungs or any other complex organ.

If the above views are correct, they will support the following propositions. That the brain substance alternates between three distinct physiological states.

1st. The state of sleep, which is complete if it embraces all parts of the brain. During this state, and this state only, nutritive regeneration of the cerebral organism takes place; and this regeneration will be universal or partial, according to the general or partial rest of the cerebral functions.

2nd. The state of wakefulness, in which the functions are alert, but not active. Like the strings of an instrument stretched up to concert pitch, but not vibrating. This state is always partial, because one or other of the cerebral functions is constantly in activity during wakefulness. It may be assumed as doubtful, but not improbable, that any portion of the brain whose function is alert, but inactive, during that time neither undergoes reparation nor decay. That it is in a condition of histogenetic equipoise.

3rd. The state of functional activity. This state, like the last, must ever be partial as relates to the cerebrum at large, and even to that portion of it which is concerned in the discharge of the intellectual and emotional

functions. That different mental states can succeed each other with wondrous rapidity is well known; but there are many cerebro-mental functions which cannot co-exist, although they may alternate with facility; others are liable to become preternaturally active, to the exclusion of that moderate and alternating activity which is especially conducive to mental health. During functional activity there is an exactly equivalent degree of histogenetic waste of the portion of the cerebrum concerned, a waste only to be repaired by nutritive regeneration during the first state, or that of sleep.

The cerebrum at large, therefore, may be in two opposite conditions; first, that of waking functional activity, accompanied by its equivalent of waste, and marked by equivalent decay of power and addition to the excrementitious phosphates; and secondly, that of functional repose, during which the waste is repaired

But a portion of the cerebrum, discharging a particular function, may be in three different states: those of rest and of activity, and an intermediate state, which may be called that of quiescence, in which it is ready to act, alert, but not active. It may be active during the repose of the greater portion of the cerebrum in sleep, it may be quiescent during general cerebral activity in waking hours.

This difference between the brain at large, and any portion of it discharging a special function, will explain those otherwise anomalous cases in which insanity, from moral causes or intellectual overstrain, has taken place without any apparent loss of sleep. These apparent exceptions are not so in reality, and the rule remains intact, that functional activity of a cerebral organ implies equivalent waste, which, if not repaired during sleep, becomes permanent, and morbid decay of the organism takes place, soon to be followed by appreciable atrophy.

It is the pathological fashion of the day to attribute all diseases to defect or perversion of nutrition. This appears to me to be an error, at least in the use of terms: and in science an error in terminology is not unimportant. I must, therefore, be excused for taking some pains to explain, that whilst so generally attributing insanity to changes accompanied by diminution in the bulk and weight of the organ, I do not consent to view all these changes as dependent upon disease of nutrition alone. The history of the life of an organ is the aggregate of the history of its component cells. In the history of a cell there are three stages: that of its growth, that of its decay, and the intermediate one of its functional activity, which is dependent upon the first, and which causes the third, of which the third is, indeed, during health, the exact equivalent. Now, nutrition is as unlike decay as income is unlike expenditure. The one may be a measure of the other, but it is not necessarily so, and in the cerebral organisation I do not believe that it is so. The cell of a gland undergoes nutrition until it is mature, when it bursts, and its activity and decay are complete and commensurate. By following an analogy not sufficiently close to be trustworthy, the history of the cerebral vesicles has been assumed to be precisely similar. Professor Carpenter says, "In the vesicular tissue which constitutes the essential part of the nervous centres, there are appearances which indicate that its peculiar cells are in a state of continual development; newly-formed ganglionic vesicles taking the

place of those which have undergone disintegration."* He elsewhere likens their development and death to those of the epidermic cells. But, notwithstanding this high authority, we possess no actual knowledge inconsistent with the belief that the cerebral vesicles have a more durable existence, that the changes affected by healthy functional activity are exhaustive but not destructive, or that the same cell may not repeatedly pass through the three vital processes of functional activity, waste, and regeneration. Normal changes may give place to abnormal ones in either of these three states, and insanity, with shrinking of the brain, may thus be dependent upon the defective nutrition, excessive function, or disproportionate and irremediable decay of the vesicular neurine. Insanity following starvation or inflammation affords instances of the first; insanity caused by the storms of passion, or by intellectual overstrain, illustrates the second; and the insanity of old age, when the elements of the body tend more strongly to form chemical compounds than to perpetuate the organic ones, may serve to exemplify the third. To use the term, disease of nutrition, in all these cases would be to abuse it. The processes of life are nutrition, functional action, and decay, and the vital changes may become abnormal, that is, disease may commence in any one of these states or stages.

The facts indicated in the Table already given, and the estimate of them which I have attempted to give, will, I trust, be thought sufficient to establish the law, that the symptoms of insanity are accompanied by pathological changes in the brain, the most prominent characteristic of which yet demonstrated is shrinking of its substance. The intimate changes of the organization which cause the shrinking, we have yet to learn, by the combined use of chemistry and the microscope. It is very probable that morbid degeneration of brain-substance, like that of muscular tissue, takes place by the running together of its organic elements into forms of hydro-carbon. It may be that the microscope will remain incompetent to detect undoubted indications of such a change in the whole of the encephalon, since death may be inevitable before changes can occur in the intimate structure of the whole organ grave enough to be appreciable by the assisted sight. But when portions only of the brain have undergone degenerative change, I have repeatedly and easily seen an abnormal abundance of oil globules; and in several such instances I have observed abundance of the peculiar crystals of cholesterine. The brain appears to afford no exception to the law, that one stage of regressive metamorphosis of animal tissues is that of fatty or oily compounds. The recent researches of Meckel and Henle, on the formation of spek-violet (a combination of cholesterine and other fats), during cerebral decay, afford strong confirmation of this opinion.

Of the specific gravity of the brain [a subject which I was the first to investigate in this country, and which has since attracted the attention of several excellent observers] it must be acknowledged that, although the large number of facts now collected do not appear in themselves to be very instructive, they at least prove the existence of great and constant differences in the condition of the cerebral matter, the nature of which must be demonstrated by other means. The average specific gravity of

* Human Physiology, p. 563.

the 63 cases in the Table is, white matter of the hemispheric ganglion, 1·039$\frac{23}{63}$; grey matter of ditto, 1·037; cerebellum grey and white matter, 1·040$\frac{1}{2}$. Dr. Sankey gives 1·0412 as the average specific gravity of white matter of seventy observations made upon persons dying without head symptoms. He gives 1·0346 as the average of the grey matter.* Meckel states that the specific gravity of the brain of the insane is less than that of the sane. MM. Leuret and Mitivié give the former as 1·028, the latter as 1·031. Dr. Aitkin, of Glasgow, has found considerable difference (1·025 on one side, 1·031 on the other) between the specific gravity of the substance on two sides of the same brain, in a case of chorea, a fact which we have not been able to verify in epileptic cases. Upon what do these differences depend? Surely they are at least sufficient to stimulate powerfully the labours of the microscopist and the organic chemist.†

I feel painfully the deficiency which exists in this paper, in the omission of observations respecting the amount of alkaline phosphates secreted in the urine. As proved by Dr. Bence Jones, in his papers in the 'Philosophical Transactions,' for 1846, the quantity of these compounds in the urine may be regarded as a measure of nervous decay. The large amount of phosphorus contained in the brain, and the wide range between the quantity of this substance stated by L'Heritiér to exist between the brains of the sane and the insane, the young, the aged, and the adult, sufficiently indicate that a knowledge of the conditions of cerebric phosphorus would greatly elucidate that of mental pathology. I have, however, found it so impracticable to determine the quantity of this substance in the urine of insane patients, principally from the difficulty of collecting the secretion, that with regret I have been compelled to turn aside from a research so promising of curious and important results.

I must conclude with some brief observations, which appear needful to illustrate the tabular form above given. The breadth of the grey matter of the convolutions given in the thirteenth column was ascertained by

* Vide No. 21, p. 241.

† Since this article has been written, I have had the pleasure and the benefit of reading the papers of the late Dr. Sims, in the nineteenth volume of the Medico-Chirurgical Transactions. These valuable papers contain nothing adverse to the opinion, that atrophy of the hemispherical ganglia cannot take place without a corresponding decay of mental power.

The fourteenth deduction is, indeed, to the effect—"That in phthisis, diseases of the stomach, and other emaciating disorders, the brain, also, sometimes undergoes a process of wasting." (p. 380.) Professor Todd adopts this opinion as regards persons who have been long bedridden, and those who have been habitual spirit-drinkers. If the cerebral physiology now current be true, atrophy in these latter cases, occurring without decay of mental power, may reasonably be attributed to shrinking of the corpora striata and thalami, and perhaps, also, of the cerebellum, owing to long disuse of their functions. It would not be easy, in a post-mortem examination, *to distinguish between an atrophied condition of the hemispherical ganglia, the seat of the purely mental functions, and a similar state of the ganglia of sensation and motion.* Doubtless, in the majority of cases of cerebral atrophy, all parts of the encephalon are implicated; but where the functions of the brain which relate to the body alone, or those which relate to the mind, are separately impaired, it will in future be desirable to look for those indications of partial atrophy we may be taught to expect by physiological science. It is not sought in this paper to maintain the proposition that cerebral atrophy never occurs without mental disease; the converse and more limited one, that cerebro-mental disease never exists for a considerable time without atrophy of the brain, forms the whole of its scope. It is, however, inconceivable that the portion of the brain, upon the due action of which the special functions which are called mental, depend, can be atrophied without those functions falling into an abnormal condition.

measuring, with a pair of hair-dividers, inspected by a lens, the average breadth of the least oblique, that is, the narrowest, sections, made by a perpendicular slice through the hemispherical ganglion. In five instances the breadth was $\frac{8}{100}$ths of an inch, of which 2 were cases of epilepsy, 1 of general paralysis, 1 of dementia, and 1 of melancholia. In one of these cases the cerebral shrinking was as much as $8\frac{1}{2}$ oz., in another as little as 2 oz. In 4 of these cases the brains were above the average weight. In 38 instances the depth of the grey matter was $\frac{7}{100}$ths of an inch; in the remainder it was $\frac{8}{100}$ths.

The measurements of the cranium expressed in the 10th, 11th, and 12th columns were taken after the removal of the scalp, in order that they might not be vitiated by differences in the thickness of the scalp, or by the abundance or deficiency of hair. The difference between measurements so made and others made before the removal of the scalp is not great; the principal being that of the circumference, which never exceeds an inch. The difference in the other measurements does not exceed half an inch each. A cranium measuring 21 in. in circumference will give a weight of brain somewhat exceeding 3 lbs., dependent, however, upon the vertical development; a small circumference with a high vertex giving as much cranial space as a large circumference with a low one.

Art. III.

Scarlatinal Dropsy. By John W. Tripe, M.D.

(*Co cluded from No. 27, p. 221.*)

We shall now describe the various forms of dropsy. These may be divided into two varieties; (1) *dropsy without albuminous urine,* (2) *dropsy with albuminous urine; which latter may be further subdivided into* (a) *the acute, and* (b) *sub-acute.*

1. Simple dropsy, or that with non-albuminous urine, is by no means so common as either form of the other variety, and supervenes usually within a few days after the disappearance of the scarlatinal rash. Its duration is usually less than that of the albuminous forms, the longest case I ever met with having lasted but four weeks, the average duration being about half that period. The danger attending it is slight; and, although fluid may collect in one or other of the serous cavities, usually of the peritoneum, yet I have never seen inflammatory visceral disease; coma, convulsions, or other symptoms of uræmia, occur, and have therefore termed it simple dropsy. In the case which lasted for nearly a month, the urine was tested almost every day, without albumen or any other abnormal constituents of the urine being discovered, save an abundant deposit of lithates, and a rather large quantity of disintegrated epithelium. In other cases a stray fibrinous cast or two have occasionally been met with. The symptoms attending this variety of dropsy are very similar to, though of much less intensity than, those characteristic of the other forms. Its ordinary course may be described as follows:—The patient becomes dull, indolent, and disinclined to move about, loses his appetite, complains of increased thirst: and his tongue, which had just

re-acquired its ordinary aspect, becomes slightly coated with a white fur, especially towards its base; and there is little, if any, increased heat or dryness of skin. In a few days the face gradually assumes the peculiar leuco-phlegmatic appearance so indicative of the disease; the mucous membrane of the lips, gums, and conjunctivæ become very pale; and the features look a little bloated (the contour of the face becoming fuller), but do not pit on pressure. The pulse is slightly, if at all, quickened, has no sharpness, but indicates diminished power, being soft and weak. In a day or two the eyelids become œdematous, and the effusion gradually involves the face, the extremities, and the rest of the body; and, after remaining for a period, varying from a few days to two or three weeks, gradually disappears. As before stated, the disease rarely, if ever, extends over a period exceeding one month from the invasion of the dropsy.

The next variety, which includes all forms of scarlatinal dropsy with albuminous urine, is of far more importance than the preceding, as all cases included in it arise from a diseased action, or from morbid alterations, of the kidney. As just stated, there are two sub-varieties—viz., the acute and sub-acute; but as the symptoms vary in degree only, being more intense in the acute than in the sub-acute, they will be included in one description: premising, however, that in the acute variety there is always more or less blood in the urine, and that it is attended with greater danger than the other.

The premonitory symptoms are tolerably well-marked, and consist in the persistence of the febrile symptoms beyond their ordinary duration, or in their reappearance after having ceased at the ordinary period. The child ceases to play about, becomes capricious and dull, his appetite diminishes, his eyes look heavy, and his pupils are dilated and act sluggishly. He also complains of thirst, languor and pain of the back, wants to make water more frequently than usual, and especially during the night; and his face gradually acquires the peculiar leuco-phlegmatic appearance so characteristic of the disease. On examining the urine, we find it to contain some of the abnormities described in the previous article.* It is also more scanty than natural, of lighter specific gravity, and of a reddish-brown or a peculiar smoky opalescent tint. After these symptoms have extended over a period of from a day or two to two or three weeks, the patient complains of a deep-seated heavy pain of the back and loins, and sometimes of a more acute pain of the thighs and testes, headache, and frequently of nausea or vomiting. The headache is sometimes intermittent, or occasionally periodic, when it is usually very intense, precisely resembling brow-ague. Painful muscular spasms, resembling those of tetanus, are occasionally amongst the premonitory symptoms. In one case of spasm of the abdominal muscles, no small anxiety was caused by their intensity and duration; the pain was most severe, and lasted for nearly three days with very frequent intermissions of a few minutes' duration. In this case the diagnosis was not satisfactorily made out, until paucity of the urine was ascertained to be amongst the symptoms, when the urine was examined, and the case cleared up.

Another set of symptoms of occasional occurrence, are those arising

* See British and Foreign Medico-Chirurgical Review, pp. 244-46.

from another affection of the nervous centres—viz., a greater inclination to sleep, without any actual stupor, the patient being quite intelligent when roused. These symptoms are usually attended with a diminished secretion of urine, and consequent excretion of urea, and frequently co-exist with other symptoms of uræmia.* Sometimes the only premonitory symptoms are languor, depression of spirits, and a gradually increasing pallidity of the face. Anæmia, indeed, forms one of the most important and invariable symptoms of renal dropsy; so much so, that many consider the alterations in the blood as the fount and origin of the disease. In other cases, the only premonitory symptoms are the leucophlegmasia, languor, and loss of spirits. After the premonitory stage has lasted for an uncertain period, the dropsy makes its appearance, usually in the eyelids, and gradually extends over the whole body, with but little, if any, alleviation of the febrile symptoms. If any indications of implication of the nervous centres existed previously, they are now very frequently alleviated, or disappear altogether; but at other times, though less commonly, they become decidedly aggravated. If the latter be the case, and the urine be lessened in quantity, unless prompt measures are used, stupor, coma, convulsions, or both the latter, may supervene, and death quickly ensue; or the case may be complicated with delirium and great restlessness, and speedily terminate in convulsions and death. These most commonly arise from the circulation of some poisonous compound in the blood,* but occasionally from the supervention of inflammatory disease of the brain or its meninges. From the same cause—viz., circulation of urea, or of a compound formed by its transformation, or of some other constituent of the urine, there is a great tendency to inflammatory disease of the serous membranes, and especially of the pericardium.

When inflammation occurs, the symptoms are at first extremely slight, consisting in an increased rapidity of pulse, anxiety, slight pain on pressing or percuting the præcordial region, and the presence of a friction-sound. I have seen several cases of this kind, which, if not detected early, speedily induce a fatal termination. The same character, insidiousness, belongs to all inflammations of the serous membranes, when they occur in combination with renal dropsy. Pain is but little complained of, and it is only by a close examination of the patient's aspect, pulse, and rate of breathing, as well as by frequent stethoscopic examination, even although we do not suspect cardiac or pulmonary disease, that it can be detected at an early period. The symptoms attending pleuritic inflammation are similar to those described as pathognomonic of pericarditis, except that the breathing is quicker, and cough is frequently present. Many other causes of death have been already pointed out,† but, as space forbids a consideration of their symptoms, I will merely mention that, besides the diseases there enumerated, laryngismus stridulus, œdema of the larynx, and œdema of the lungs, have each caused death in my practice.

Should the disease progress favourably, the general health improves, the thirst diminishes, the countenance recovers its healthy aspect, and the urine increases, contains less albumen and other abnormities, more urea and other normal constituents, becomes of higher specific gravity, and

* See Uræmia, Art. I. p. 24. † Art. I. pp. 241, 242.

gradually assumes its normal pale-sherry tint. On the other hand, when the disease takes an unfavourable course, symptoms of one or other of the previously enumerated complications supervene, and death ensues; or the acute stage may degenerate into the chronic, when the urine becomes of low specific gravity, of a pale greenish tint, and contains far less saline and organic compounds than in health.

Before concluding this section of our subject, I will again draw attention to the rapidity with which organic diseases of the parenchymatous organs, or of their serous coverings, set in; to the insidiousness of their course, and to the great danger attending their presence. As an illustration, I may mention a case of endocarditis (which was fatal in less than three days), in which the only early symptoms were cough, a greatly increased rapidity of breathing, and a very slightly accelerated pulse, without any abnormal pulmonary or cardiac sounds being detected, although a careful stethoscopic examination was made. On the evening of the second day, a marked irregular action of the heart came on, and a mitral regurgitant and direct aortic bruit were very audible. In twenty-six hours after their detection the patient died, having walked about a quarter of a mile to my house on the evening of the second day.

Diagnosis.—The only positive evidence we can have of the case being one of *scarlatinal* dropsy, is proof of the patient having been exposed to, or having suffered from, the scarlatinal virus; but in many cases of inflammatory dropsy it is impossible to assign any special cause. The peculiar desquamation of the skin after an attack of scarlet fever, even in those cases in which no rash has made its appearance, will often of itself afford satisfactory evidence of the nature of the attack. But it is sometimes far more difficult to diagnose the disease in its first stage before dropsy makes its appearance, and when the history leads us away from the supposition of the patient having suffered from the fever. In one case of spasmodic contractions of the abdominal muscles, simulating tetanus, the diagnosis was made out only by the peculiar state of the skin exciting suspicion, and leading to inquiry about the renal functions; and the same occurred in another case of uræmic poisoning, in which the patient (a girl of 15 years) was seized, as the parents stated, with convulsions like epilepsy and coma, without having suffered from any previous illness. In both these patients there was suppression of urine. The *diagnosis* of this latter class of cases from convulsions produced by other causes, meningitis, epilepsy, apoplexy, typhus, and narcotic poisoning, must be based on—(*a*) the previous history; (*b*) the state of the skin; (*c*) the presence of ammonia in the expired air (Frerichs); (*d*) a careful inquiry into the state of the urinary secretion, including chemical and microscopic examinations; (*e*) and a careful comparison of the symptoms present with those of the suspected diseases.

The detection of albumen alone is not a sufficient foundation for making a certain diagnosis, as there are several medicinal agents, and other ingesta, which, in certain conditions of the system, or when taken for some time, will induce albuminous urine. Albumen has also been detected in the urine during the course of several febrile inflammatory diseases; but there is no other febrile disease which will cause its excretion after the inflammatory stage has completely passed. Nor is the detection

of renal epithelial cells alone a more certain guide, as the long-continued exhibition of diuretics will lead to their presence in the urine. And we should expect this would happen, as we know that any agent which excites an abnormal activity of the renal secretory cells diminishes the duration of their existence and induces desquamation before their full growth is attained. The only certain guides as regards the urine are the presence of albumen and fibrinous casts, or of the combined presence of the following other departures from a normal state, viz., the presence of renal epithelium, a diminution of its solid constituents, especially of urea, and a diminished quantity and altered colour of the urine.

Prognosis.—The prognosis will materially depend on the presence or absence of organic disease. If inflammatory disease of any organ exist, the prognosis will be unfavourable in proportion to the importance of the organ affected, and the extent and severity of the disease. The organic disease most fatal in my practice has been meningitis, and the next, endo-pericarditis (for the fatality of complications, see former article, pp. 241-2). If there be no visceral disease, the state of the urine affords the most definite information. To arrive at a sure prognosis from the urine, we should examine it at certain regular intervals of time, and compare each result obtained with those noted at the previous examinations, when, if we find its quantity, specific gravity, and the amount of saline constituents to increase daily, the albumen, fibrinous casts, and blood corpuscles to diminish, and its smoky or dark colour and opalescent tint to gradually change into its normal pale-sherry colour, our prognosis should be favourable. On the other hand, if its quantity, the total amount of saline constituents excreted, and its specific gravity be diminished; or, at any rate, if the total amount of solid constituents in the twenty-four hours be less; if the fibrinous casts, albumen, and blood corpuscles be increased; if the smoky colour and opalescent tint become more marked, we may be sure that our patient is getting worse. There is another condition of urine which is indicative of a greater amount of renal disorganization even than the last, viz., when the urine gradually becomes clearer, lighter in colour, less albuminous, and contains also less blood-corpuscles and fibrinous casts, but, at the same time, its specific gravity diminishes, and the amount of saline compounds contained in the urine passed during the twenty-four hours slowly but regularly decreases. After a short time this kind of urine assumes a peculiar greenish tint, froths much when shaken, and contains a variable, but usually a small, quantity of albumen. We must be extremely cautious in giving a favourable prognosis in these cases, as they frequently progress to an advanced stage of renal degeneration. The continued leuco-phlegmatic appearance of the patient would lead us in these cases to suspect this termination, even without an examination of the urine.

Treatment.—Few authors perfectly agree in treatment to be adopted in scarlatinal dropsy, some advising a strictly antiphlogistic plan in all cases, others, tonic astringent remedies even from the first; and, as usual, the truth seems to lie in the mean between the two extremes.

Prophylactic Treatment.—As the dropsy usually follows the milder forms of the fever, there is but little doubt that proper treatment; medical and dietetic, subsequent to the eruptive period, would prevent many

attacks, as doubtless almost the majority result from improper management. It is, therefore, a matter of considerable moment to powerfully impress on the parents the necessity of paying great attention to the functions of the skin, and to diet, and also of guarding against atmospheric vicissitudes. The most important matter to be attended to after an attack of scarlet fever, is to encourage the proper action of the skin. For we know that the skin and kidneys are, to a certain extent, vicarious in their action, each excreting saline particles, organic compounds, and water. A warm bath, containing, according to its size, one, two, or three ounces of common washing soda, or carbonate of potash, should be used twice a week, followed by a good rubbing with a coarse towel; or the patient may be well rubbed all over with yellow soap before using the bath. A bland nutritious diet, chiefly composed of farinaceous food, eggs, and milk, with occasionally a little broth, should be taken, so that the kidneys may have little else to excrete than the compounds resulting from the disintegration (oxidation) of the ultimate elements of the body. I know that in making this observation I am invading disputed ground, but the observations of Lehmann and others, show that the nitrogenized elements of the urine are in a near proportion to the quantity of nitrogenized food taken. There is certainly, chemically, some difficulty in the way, but not enough to overturn this view, which is supported by most recent observers, including Frerichs. It is also probable, as supposed by Prout, that if imperfectly formed albuminous compounds are absorbed into the circulation, they are excreted in the form of lithate of ammonia. Stimulating drinks of all kinds are also to be strictly avoided, and especially spirits. An occasional aperient, and a sudorific at night twice a week, will also be found advantageous. But if, in addition to all these precautions, we do not protect our patient from draughts of cold air, and from the vicissitudes of our variable climate, all our care will be lost. As proof of this statement, I refer to tables 4 and 5, where we see that the proportionate and absolute mortality from the disease is least in that month of the year, August, which presents less atmospheric changes than any other, and that it rises in each of the following months, when the changes become greater. It is, therefore, evident that, in changeable and cold weather, confinement to one room kept at an equable temperature is necessary, and also that, whatever the weather may be, warm clothing, and especially the use of flannel next the skin, are indispensable to the success of prophylactic treatment. These precautions should be rigorously enforced during the first three weeks of convalescence, or certainly after the fading of the rash, and may then be less carefully observed during the fourth week, and afterwards abandoned. That the periods here assigned are not too long, may be seen by examining table 15, which shows that 47·6 of all the attacks happened during the first fortnight, 82·3 per cent. during the first three weeks, and 93·8 per cent. during the first month.

The treatment of the non-albuminous variety of the dropsy is very simple, our chief indications being to restore the tone of the system, increase the red corpuscles of the blood, and remove the excess of water. To effect these, a combination of slight diuretics with the tincture of the sesquichloride of iron, and the occasional exhibition of a hydragogue purgative, have proved very effectual. My favourite combination con-

sists of tincture of digitalis or vinegar of squills, spirits of nitre, and tincture of sesquichloride of iron; and a dose of compound jalap powder every other or every third morning.

The treatment of the albuminous variety requires much greater care and consideration, and varies greatly in the febrile and post-febrile stage, and also according to the intensity of the former. Before deciding on any plan of treatment, we must carefully investigate the condition of the urine as it is our best and most certain guide. In practice, I divide the febrile cases into three classes: (*a*) into those in which the urine is scanty and bloody; (*b*) into those in which the urine is scanty, but not bloody; and (*c*) into those in which the quantity of urine is but little diminished, and there is no blood present. The first variety (*a*) is usually of the most acute kind, and is indicative of considerable disease in the secreting part of the kidneys. The treatment required here is at first decidedly antiphlogistic, both as regards diet and medical appliances. Our first step will consist in the abstraction of blood from the loins by cupping or leeching, to an amount, varying according to age, of from two to six ounces, remembering that our object is not to cure the disease by bleeding, for we cannot effect this, but to relieve the present extreme congestion, and prevent the occurrence of further mischief. After having abstracted a little blood, our next indications are to restore (1) the functions of the skin, and (2) the normal state of the circulating fluid; (3) to relieve still further the congestion of the kidneys; (4) to afford tone to the distended renal vessels; (5) to remove the dropsical effusion; and lastly (6), to adopt such other measures as the peculiar state of our patient, any idiosyncrasy of constitution, or the presence of complications may require.

The first indication—viz., *to restore the functions of the skin*, must be effected by keeping the patient in bed, or confined to one room; in preventing him from feeling the changes of weather; and in the exhibition of antimonial sudorifics. Saline sudorifics must be carefully avoided. A warm bath, containing common soda or potash, followed by long-continued friction with a coarse towel, should be frequently used. This plan of treatment is of very great service in all stages of the disease, and especially when uræmic symptoms are impending or actually present, as they will sometimes induce free perspiration, and thus relieve the blood of part of its superfluous fluid, and of the urea and other salts which should be excreted by the kidney. Another powerful remedy consists in wrapping the patient in a sheet dipped in warm or cold water, and then covering him with blankets, carefully watching, if cold water be used, that faintness do not happen. I would not recommend the cold sheet in any but extreme cases, for fear of inducing congestion or inflammation of the lungs, and lest the vital powers should not be sufficiently strong to induce reaction.* In addition to these measures, warm clothing should be adopted, and especially flannel next the skin.

* A friend of mine, Mr. E. May, related to me a most successful case of "cold packing." The patient had been under his care, and under that of another medical practitioner previously, for some time, with symptoms of effusion into all the serous cavities, dilated pupils, and coma. All the ordinary remedies were tried, including diuretics, and without effect, as, except with a motion, no urine was passed, even if twenty-four or thirty-six hours elapsed. The child was wrapped in the cold sheet, and then covered with blankets, which induced in ten minutes most profuse perspiration, and was followed by slow but steady recovery.

The next indication, *to restore the purity of the circulating fluid*, is most important; and to effect this, we have to eliminate the urea and other organic compounds which have accumulated from the diminished action of the kidneys and skin, and also to restore the proper proportion of the red corpuscles and albumen. To eliminate the foreign matters, we must restore the functions of the skin and kidney, and set up increased action of the intestinal glands, by which some of the excrementitial compounds of the body are normally excreted. The object of purgatives in this form of dropsy is not merely to drain off the accumulation of water, but also to remove these compounds from the blood; and we must, therefore, not merely use hydragogues, but occasionally rhubarb, jalap, aloes, or other purgatives which will produce the effect desired. A most useful medicine is the compound jalap powder, given daily in full doses, with the substitution once a week of a dose of calomel and rhubarb, the proportion of the former being very small. We must remember that the intestinal glands excrete compounds which are chiefly, if not entirely, unnitrogenous, so that they cannot take on an action vicarious with that of the kidneys. To restore the functions of the kidneys is often by no means easy of attainment, and the treatment must vary according to the amount of renal disease existing. When the urine is bloody, after the local abstraction of a little blood, we have to attend to the two next indications—viz., to relieve still further the congestion of the kidneys, and to afford tone to the distended renal vessels. The most effectual way to fulfil the first of these is to act powerfully on the skin by the means previously enumerated; to apply dry cupping, stimulating embrocations, and other counter-irritants to the loins. The use of epithems of turpentine every other day, or of embrocations containing turpentine, is often attended with very beneficial results; mustard poultices are sometimes useful. The application of blisters as counter-irritants must be carefully avoided, unless required for some inflammatory disease of the viscera or their coverings. This remark applies with increased force to setons or issues.

The medicines which we exhibit should be selected to fulfil both the indications here pointed out, and to produce a tonic or constringing effect on the distended renal vessels. In the early stage of the disease, the plan which I have found most effectual has been, to exhibit the tincture of the sesquichloride of iron in full doses, in combination with a few minims of tincture of digitalis, or by itself; the digitalis being added in those cases only where there is an increased rapidity of pulse. When blood has ceased to be passed, and the other abnormities have diminished, should the patient not progress sufficiently rapidly, the exhibition of tannic acid in full doses is eminently useful. I have also, in an advanced stage, found much benefit from the free use of alum, with or without cubebs. Some have recommended gallic acid in preference to tannic; but Frerichs and others prefer the tannic. The object of these astringents is not merely to prevent the exudation of albumen, but to relieve the congestion of the gland, by producing tonic contraction of the capillary vessels. The great reason for preferring the tincture of the sesquichloride of iron to any other astringent, is because it assists most materially in restoring the blood to its normal condition. Thus it has

been shown, that the blood is always more or less anæmic in these cases, the proportion of the red-corpuscles being very much diminished; and while, therefore, we are relieving the blood of its accumulated effete particles, and of its excess of water, we should attempt also to increase the number of blood-discs. A compound of tannic acid and lactate, or ammonio-citrate, or potassio-tartrate of iron, is very valuable, as, when combined with the use of bitartrate of potash, or some other hydragogue, given at proper intervals, it fulfils all our indications. We must in all cases remember the necessity of exhibiting iron in scarlatinal dropsy, as one or other of its preparations are almost invariably required. The diet should be bland and nutritious, care being taken that, whilst we support our patient, we do not overload the stomach, and should therefore recommend eggs, milk, farinaceous food, with a little meat, and less vegetables.

Our next indication—viz., *to remove the dropsical effusion*—is one of some moment, and requires care in its fulfilment, but must not be attempted at the expense of neglecting any other indication. Purgatives, as already pointed out, are the most powerful agents in effecting this, and especially hydragogues, the type of which, and the most useful—elaterium—may be administered every other morning, the dose being carefully regulated according to the age of the patient. Another most effectual purgative is the compound jalap powder, given in full doses every morning, in a moderate quantity of fluid; for we must remember, in administering saline purgatives, that their action depends on the specific gravity of the dose given; for if a saline aperient be administered in a quantity of fluid sufficient to reduce the specific gravity of the dose below that of the serum of the blood, it will be absorbed into the blood, and not act as a purgative.

The period at which diuretics should be commenced is a point somewhat in dispute; but it is quite certain, if they are administered too soon, that they will induce bad consequences, by adding to the diseased action. A consideration of the action of diuretics is too large a subject to treat of here, but it is pretty well established that many diuretics, whilst they induce an increased flow of water, diminish the quantity of the saline constituents passed in a given time, and might, therefore, very well be termed hydragogue diuretics. As proof of this, we may refer to Professor Krahmer and Dr. Golding Bird, who state the quantity of water and solid constituents passed in a corresponding period of time, previously to th exhibition of diuretics, and whilst the patients were under their influence. Also to the experiments of Dr. Parkes* with liquor potassæ, which were performed partly on himself and partly on some of his hospital patients, as well as to the researches of others, which show that the albuminous compounds of the blood are acted on by it.†

A careful consideration of the facts adduced by these and other authors lead to the following conclusions, viz., that juniper, squills, and turpentine, if not all the vegetable diuretics, are hydragogues, and, therefore, suited for removing dropsical collections, unless we wish, at the same time, to induce an increased excretion of the saline constituents of the urine. If, however, we have any reason to suppose that the blood is charged with the

* British and Foreign Medico-Chirurgical Review, vols. xi. & xiii.
† See an admirable summary in Carpenter's Physiology, fourth edition.

nitrogenized compounds which result from the disintegration of the fluids or solids of the body, we must then combine them with one or other of the alkalies or their compounds, strictly avoiding the exhibition of liquor potassæ on account of its action on the albumen of the blood. But in our use of diuretics, we must be chiefly guided, not by the amount of the dropsical effusion, but by the state of the kidneys, for if these glands are the seat of much congestion, whether active or passive, diuretics *per se* will prove injurious, by affording increased stimulation to an already over-stimulated organ. The presence of blood in the urine is, as a rule, a direct contra-indication to the use of saline diuretics, and, in my opinion, of any except digitalis, colchicum, or spirits of nitre, either of which, combined with tincture of the sesquichloride of iron, or tannic acid, is useful. When the urine ceases to contain blood, and the quantity of epithelial casts and scales is less, saline diuretics are very useful, and in some cases, especially the preparations of potash in combination with squills, guaiacum, colchicum, or broom. If the disease should degenerate into the chronic form, the free use of Vichy, Seltzer, or other similar water, is often beneficial, the former especially, for they seem to wash, as it were, the fibrinous casts out of the uriniferous tubules. Those waters which contain alkalies also act on the effete particles contained in the blood.

Having bestowed so much space in considering the general principles of treatment to be followed, it will be unnecessary to point out every variation required in the different forms of the disease. The more acute the disease, the more energetic must be our treatment, care being taken lest the *nimia diligentia medici* be not exercised injuriously. We should not think of using blood-letting in any but those cases in which the urine is very scanty and bloody, or unless imminent symptoms of uræmia were present, with scanty urine; and it would, therefore, be inadmissible in those cases of dropsy included in varieties (*b*) and (*c*). The former (*b*) may be treated by alkaline warm baths, and subsequent friction of the skin, sudorifics, preparations of iron, or of some other renal astringent, with or without vegetable diuretics (excluding squills in the early stage), hydragogue purgatives, terebinthinate applications to the loins, a carefully-regulated diet, with protection from atmospheric vicissitudes. In variety (*c*), diuretics can be used earlier and more safely than in the other forms; baths are not frequently required; and the exhibition of tincture of the sesquichloride of iron, with hydragogue purgatives twice or three times a week, will usually induce a satisfactory result in a short time.

The treatment of the *complications* is most important, and varies considerably from that which we should adopt if they existed independently of the dropsy. The frequent occurrence of inflammations of the serous membranes in cases of renal dropsy was particularly pointed out by Christison, who also noticed that the renal disease exercised a considerable modifying power on the action of some medicines, and especially on that of mercury. Under the head of *Causes of Death*, we have shown that all the complications may be included under two heads, the accidental and pathofusical;* and we shall adopt the same division in considering the subject of treatment, premising a few observations on the modifying influence of the renal disease on our remedies.

* See Art. I. p. 241, note.

Blood-letting, as a rule, must be small in quantity, and as much as possible avoided, for in these cases the normal proportions of the blood-corpuscles are materially diminished. As pointed out, this has been shown by chemical analysis, and by the peculiar leuco-phlegmasia of our patients. To remove blood unnecessarily would, therefore, exert a most prejudicial influence. General blood-letting should, therefore, be used sparingly, both as to amount and frequency; and local blood-letting, either by cupping or leeches, is decidedly preferable, care being taken not to allow the draining from the leech-bites to go on unnoticed and unwatched, as we might thus abstract more blood than we wished. We must remember that we cannot remove, although we may reduce, the amount of the *materies morbi* by blood-letting, and must, therefore, to effect this, trust chiefly to other evacuants. The use, however, of blood-letting in serous or parenchymatous inflammations, is undoubtedly beneficial, and in those of the pulmonary or circulatory organs often relieves the extreme oppressions of their functions; but, so far as my experience goes, it is not of equal service in cranial affections. Should we meet with inflammation of any organ, and an almost total cessation of the renal functions, I would prefer abstracting the blood from the lumbar regions rather than from the locality of the inflammatory disease. The object sought, as regards the kidneys, by blood letting, seems to me rather to be that of relieving congestion, than of stopping inflammatory disease.

The exhibition of mercury, even as a purgative, must be very sparing and careful, as this remedy often produces its peculiar effect (salivation) from very small doses, and in a very short time. I have seen profuse salivation result from one-grain doses of calomel given at intervals of four hours, and even from a single dose given as a purgative. In 1842-3 I witnessed several cases, in hospital practice, of most profuse salivation from moderate doses of mercury in pericarditis complicating Bright's disease. And not only is the use of mercury open to this objection, but it does not so certainly or readily produce its ordinary effect of inducing the absorption of the effused fibrin,* or of staying its further deposit. I always treat inflammations of the parenchymatous organs, or their serous coverings, with mercury, but am very careful as to the dose and frequency of exhibition, and would advise the case to be visited at least twice a-day, and the remedy to be discontinued, at any rate, for a time, directly the red line shows itself on the edge of the gums. I have also seen pericarditis ensue, in several cases, after the patient had been salivated for other diseases.

We must also be very careful in the use of blisters, especially in children, as frightful sloughing sometimes follows their use. I saw one case of most extensive sloughing of the thoracic parietes, which resulted in death, follow the injudicious application of a blister. I say injudicious, because it was kept on too long; indeed, in children, we should never keep them on in any disease, and especially in this, until they produce vesication.

* In thus expressing myself I must not be supposed to express a belief that fibrin once poured out of the vessels and coagulated can be reabsorbed without undergoing any change; for recent researches prove that in many, if not in all, cases, it is first converted into a kind of fat, with the evolution of ammonia, and that the two then combine to form a kind of soap, which is dissolved in the effused serum, and then absorbed.

The *diet* should also be less strictly antiphlogistic than in ordinary disease, as the red-corpuscles of the blood suffer such extensive diminution, and the powers of the system are so much reduced. Indeed, it is a question, in some cases, how far this alteration in the blood is to be looked on as one of the causes of the inflammatory attack.

Without treating specifically of the action of any other remedies, I will conclude these preliminary considerations by again observing, that in curing the complications we do not remove the *fons et origo mali*, and do not prevent the occurrence of similar complications in other organs.

Treatment of Cranial Disease.—We must be very careful not to treat any case of cerebral disease as inflammatory until, by careful examination of the symptoms, we have assured ourselves that the symptoms are not those of uræmia. Our analysis of the causes of death in 128 cases shows that 6 were from meningitis, and 7 from cerebral effusion. We must, therefore, if possible, distinguish between uræmia, cerebral effusion, and meningitis: I say if possible, because the pathognomonic symptoms are often very obscure in these cases, especially in those of meningitis; still, with care, they can be separated. Having satisfied ourselves that the case is one of meningitis, we must abstract a little blood locally, apply cold to the head, blister the temples or back of the neck, exhibit mercury until it produces the red line, and should adopt the other treatment pointed out for relieving the renal disease and the abnormal state of the circulating fluid. In cases of cerebral effusion, local treatment seems of little avail, still we might try the effect of moderate leeching and of blisters. The treatment of uræmia will be considered after that of the inflammatory complications.

Disease of the Larynx is rare, but very fatal. I have lost several cases from (*a*) laryngitis, (*b*) laryngitis with effusion of lymph, and (*c*) œdema of the glottis. The treatment of the former should be by antimony, in free doses, combined with opium or syrup of poppies, local blood-letting, blistering, and, perhaps, mercury: of the second (*b*), by more free and *early* depletion, mercury, blisters, and the exhibition of sulphate of copper in emetic doses every hour and a half or two hours; mercury seems to exercise little influence here, as it is not speedy enough in its action. Blistering by caustic ammonia or boiling water may be adopted, and also painting the back of the fauces, the glottis, and larynx, with a strong solution of nitrate of silver (℈ij. ad ℥j.), is in this, as well as in œdema of the glottis, a most useful application. Œdema of the glottis must be treated by the nitrate and by scarification, conjoined with tonics and stimulants.

Disease of the Lungs and Pleuræ.—There is often much discrimination required for the early detection of thoracic inflammations, and yet it is a most important matter, as no less than 40 of the 128 cases were fatal from thoracic disease, and of these 40, 75 per cent., or nearly 22 per cent. of the 128 cases, were fatal from inflammatory disease of the heart or lungs. The principles laid down for the treatment of inflammatory complications apply forcibly here, as the exhibition of tartar emetic, in full doses, is not nearly so useful as in ordinary pneumonia. The most successful plan I have found to consist in the careful exhibition of small doses of mercury with ipecacuanha, followed, if hepatization should ensue, by small doses of

iodide of potassium, combined with opium and ipecacuanha. Opium, or syrup of poppies, is also very valuable. The treatment of pleuritis should be very similar; and in the advanced stages of either—that is to say, after the acute stage has passed—the judicious application of blisters is often attended with very good results.

Heart and Pericardium.—Diseases of the heart and its covering are very fatal when they occur, and are very insidious. We may treat pericarditis by local depletions, calomel with ipecacuanha, or antimony, preferring the former if there be much constitutional depression; followed speedily by a large blister to the præcardial region. I have been successful in two cases, and unsuccessful in one, which was otherwise complicated. Endocarditis may be treated by calomel and blisters. Dropsical effusion into the pericardium is sometimes excessive, and it becomes a question whether, instead of trusting to hydragogue purgatives and other general measures to drain off the fluid, we should tap the pericardium. I have never seen this done, but think with care that it may be readily performed. We must also be careful not to depress the system too much by whatever remedies we use, as the symptoms of pericarditis may continue after all necessity for active treatment has passed by, and when the exhibition of wine, rather than of antiphlogistics is required.

The Treatment of Diseases of the Abdominal Organs and the Peritoneum varies according to the organ affected; that of peritonitis should be similar to that just indicated for pericarditis. Chronic diarrhœa, as it usually depends on ulceration of the intestines, must be treated accordingly, by the administration of sulphate of copper, in as large doses as the stomach will bear, either by itself, or combined with small doses of ipecacuanha, or of the tincture of the sesquichloride of iron. A large blister should also be applied to the abdomen, and repeated, if necessary. Should these not be successful, the other ordinary treatment for ulceration of the intestines must be adopted. Cases of ascites without inflammatory disease, which are rare, require only the ordinary treatment for dropsy, with counter-irritant and absorbent applications to the abdomen, such as compound tincture or ointment of iodine.

The other complications, gangrene, sloughing, abscesses, erysipelas, &c., will require the treatment ordinarily adopted in such cases.

The Treatment of Uræmic Poisoning must materially depend on the urgency of the symptoms, as in some cases danger is imminent. Thus, when we have to treat a patient in a comatose state, or in convulsions, with total, or nearly total, suppression of urine, if we lose time, we lose our patient. Warm baths, or packing in a sheet previously dipped into cold or warm water, and then enveloping our patient in a blanket, are measures which must be immediately resorted to. Hydragogue purgatives, cupping or leeching the loins, and, in some cases, diuretics, are most useful. We may also employ frictions of the lumbar and abdominal regions, with terebinthinate embrocations, or turpentine stupes. Frerichs recommends the use of dilute hydrochloric acid to neutralize the ammonia which he believes to be circulating in the blood; and states his belief that the acid passes into the blood, combines directly with the alkali, and is then excreted by the urine; and he also recommends the patient to be washed with vinegar. The question of depletion and counter irritation

in these cases is often attended with doubt, and requires some care in making our decision. If we have reason to believe that the symptoms arise from congestion of the brain, we should abstract blood from the temples or scalp, and apply frigorific lotions and blisters; but as post-mortem examinations do not ordinarily reveal any organic cerebral changes, or marked congestion, we are not, as a rule, justified in adopting such measures, and should rather trust to our other remedies. Slight cases do not require active treatment. A combination of citrate of iron and tincture of digitalis or squills, occasional baths, terebinthinate embrocations used daily, and occasional purgatives, will suffice.

I cannot conclude this brief outline of the treatment to be adopted in uræmia without observing that the old adage, "prevention is better than a cure," applies most forcibly here. For the history of uræmia leads to the conclusion that proper and effectual measures for promoting the functions of the skin, with due attention to ventilation and diet, would in most cases prevent its supervention. And these prophylactic measures should be adopted not merely to prevent the severer effects of uræmic poisoning, but the slighter—viz., mal-aise, pseudo-neuralgic pains, head-ache, and other anomalous aches and pains which are frequently referred to all but the right cause. Indeed, I have no doubt that an extended and careful investigation of the symptoms and treatment of uræmia, not only as a sequela of scarlet fever, but in its relation with other diseases, will clear up many an obscure point in pathology.

Art. IV.

*A Nosological Inquiry into the Acute Diseases of the Chest known to the Greeks—viz., the Pleuritis and the Peripneumonia; with Remarks upon the Peripneumonia Notha of the Moderns.** By W. T. Gairdner, M.D., Lecturer on the Practice of Medicine in Edinburgh.

The very important position assigned, in all ancient nosological descriptions, to the disease variously called *pleuritis*, *ἡ πλευρίτις (νόσος)*, *το νόσημα τας πλευρᾶς*, *ἡ πόνος του πλευροῦ*, by the Greeks, and *morbus lateralis* by the Romans, shows that this affection (*the disease or pain of the side*) is by no means one of those which has been left for modern ingenuity to discover or appreciate. Nor does it seem possible to doubt, that the ancient *pleurisy* had, to say the least, as wide a range, and as great a fatality, as the modern. Moreover, the conformity of most of the early descriptions with each other, and with facts which any one may now observe, as well as the numerous speculations as to the nature and seat of the disease, show that great care had been bestowed upon its diagnosis, and that no little discussion had taken place as to its more recondite anatomical characters. And it is likewise a most remarkable fact, of which we have ample evidence, that throughout an epoch of history when the "right of private judgment" was more freely exercised than it has, perhaps, ever been since, in matters of medical practice, and

* This paper may be considered as an appendix to the memoir on Collapse of the Lung in its Relation to Modern Practice, the first part of which was published in this journal in January, 1854.

when the general theory of medicine was perplexed by multitudes of opinions little less varied and discordant than those current in the present day, the general treatment of pleurisy maintained a steadiness and consistency that leave us, in this nineteenth century, little cause for self-gratulation. Here, then, is a case in which, if ever the experience of the past is to be useful to us, it ought to be worthy of our most careful study and conscientious appreciation.

If, now, we attempt to reconcile the ancient idea of pleuritis with that of almost any author of the present day, we shall be struck by a discrepancy, to which I have elsewhere alluded in the case of pneumonia, and which arises directly out of the ancient as compared with the modern point of view. The rational or subjective characters—the symptoms—all the attributes, in short, of pleuritis, which constitute its essence *to the sufferer*, were combined by the ancient physicians with a few well-marked phenomena of a less obtrusive kind, into a definition which, to their view, *marked the disease*. The definition, or general idea, thus established, was subject to no such exceptions or irregularities as we are now led to attribute to the disease we call pleurisy. There was, for instance, no question of diagnosis or treatment relating to *chronic* or to *latent* pleuritis. The latter had no existence in nosology before the era of Laennec; the former may have been known, indeed, but known only under some other name. *Pleuritis* without pain, fever, cough (or even devoid of any one of these) would have been disowned equally by Hippocrates and Celsus, by Galen and Avicenna, by Sydenham and Huxham. Yet such a form of pleurisy is now not only recognised in science, but is specially urged upon the attention of our students as having the characters of an inflammatory disease, the detection and treatment of which, by means of physical signs, forms one of the triumphs of modern medicine. And, inasmuch as it is generally admitted that all the rational symptoms (with trifling exceptions) may be absent in the most acute pleurisy, it is plain that the tendency of modern studies is to thrust into a corner, if not to dismiss from consideration altogether, those attributes of this disease upon which, for at least twenty-one centuries, its position in the nosology, its diagnosis, its prognosis, and its relation to remedial measures, have been conceived absolutely to depend.

The importance of this consideration will not be denied; it may, however, be maintained that I exaggerate the practical difference between the ancient and modern pleuritis, by pressing an argument founded on exceptional cases. In the view of this objection, and in order to bring into view some of the more important points of difference, I propose to pursue this subject a little further.

The pleuritis of the ancients was essentially a disease of short duration. In the Hippocratic writings it is constantly described as one of the fevers, amenable to the supposed laws of crisis and critical days. In the description of the well-known case of Anaxion,* the only *clinical* history of pleuritis which can be certainly ascribed to Hippocrates himself, the disease is called an *acute fever*, and the appearance or non-appearance of critical phenomena is particularly noticed on the seventh, eleventh, seventeenth, and twentieth days. On this last day the patient was,

* Epidem. III. 35, case 8.

without fever, but continued to have "thirst, and no good pulmonary purgation" (expectoration).' The final crisis took place on the thirty-fourth day. From the other authentic writings of Hippocrates, it would appear that this case, though terminating within the forty days which constituted the ordinary limit of acute fevers, must have been regarded by him as unusually protracted. In the 'Aphorisms,' v. 8, he remarks, that "in the pleuritics who are not purged (*ἀνακαθαίρονται*—i.e., who do not expectorate) in fourteen days, the disease is changed to an internal abscess or *empyema* (*τουτέοισιν ἐς ἐμπύημα μεθίσταται*)." And again in 'Aphorisms,' v. 15, he remarks that "those who pass from a pleurisy to a *suppuration* (*ἐκ πλευρίτιδος ἔμπυοι γίνονται*) get well if they are purged within forty days from the rupture (meaning, probably, the first considerable expectoration); but if they are not purged within this time, they change to *phthisis* (*ἐς φθίσιν μεθίστανται*)." In the 'Prognostics,' and in various parts of the less authentic Hippocratic writings, as in the treatises 'De Morbis,' the reader will find descriptions in accordance with these passages; but he will hardly apprehend the full signification of them, unless he be aware that *empyema* (an internal suppuration) is universally defined as a chronic disease by the Greeks; and that this word, besides a limited application in accordance with our present use of it, comprehends all the cases in which there is a long-continued discharge of pus, or in which there occurs a collection of matter, attended by hectic fever, in any internal organ, and especially in the lung. This disease was regarded as perfectly distinct, in its essence, from pleurisy, although sometimes grafted upon it; and, indeed, the majority of cases of pulmonary empyema (which was also called *vomica*) were more allied to phthisis, from which, however, it was likewise distinguished by most writers. All the Greek authors, moreover, agree with Hippocrates, in limiting the application of the term pleuritis to those early stages and rapid forms of disease which may be called truly acute; and which, occurring in previously healthy individuals, either ran their course within a few weeks, or underwent such changes of type as, in the opinion of these writers, converted them into different diseases. Hence an empyema was not regarded as the natural result of a pleuritis, but as a metastasis, produced (in the language of the humoral pathology) by an imperfect coction of the febrile matter, and the consequent absence or imperfection of the critical evacuation. I refrain from further quotations in proof of these statements, and will merely refer the reader to a series of passages, to some of which I shall have to allude repeatedly hereafter.*

It can scarcely be necessary to give any references to modern authorities in order to prove that the modern pleurisy does not, as respects duration, correspond in any degree with the idea formed of this disease

* Aretæus Morb Acut. Caus. &c., Lib. I. chap. 10; Morb. Diaturn Caus. &c., Lib. I. chap. 8, 9. Cælius Aurelianus, Morb. Acut., Lib. II. chap. 13—17 inclusive; Morb. Chronic., Lib. II. chap. 14, and Lib. V chap 10. Paulus Ægineta, Lib. III, § 22, 23, (he does not distinguish empyema from phthisis). Numerous references will be found in the excellent edition of Dr. Adams, to all the other authors of the classical period. I am happy to be able to strengthen the above views in regard to the empyema of the ancients, by the authority of this truly learned and able physician, to whom the present age lies under deep obligations for his great efforts on behalf of ancient medical literature. The note of Dr. Adams on the Prognostics of Hippocrates (Sydenham Society's edition, vol. i. p. 246), as well as the singular confusion of Mr Littré's commentaries on this subject. had escaped me till after the above passage was written.

by the ancients. If certain cases of pleurisy follow this course, it will be generally admitted that a very large proportion of those which are fatal by effusion have a much more protracted duration.

Pain, as a symptom of pleurisy, is common to many cases of the ancient and modern disease. Moreover, the ancient and modern symptomatology agree in recognising mere pain, apart from the other symptoms, as not characteristic. The distinction of pleurisy from certain painful affections, supposed to be of a different nature, and to depend on an inflammation of the intercostal muscles, was clearly made by Cælius Aurelianus,* as well as by the later Greeks;† it seems, however, probable that this distinction acquired much greater vogue after the revival of letters, when the contrasted names, *pleuritis vera*, and *pleuritis notha* vel *spuria*, became established.

The chief diagnostic signs referred to by Galen (*see note*) as distinguishing the real pleurisy from the muscular affection, are the presence of cough, even where there is no expectoration; the hardness and tension of the pulse; the severity of the fever and dyspnœa; also the absence of tenderness on pressure, which he gives as a character of the muscular pains. It is sufficiently evident, however, that Galen must himself have sometimes mistaken the true pleurisy (speaking *anatomically*) for the muscular affection. He says of the latter that "when the phlegmon is concocted, unless the pus is discussed, it comes to the surface, and is opened" (*loc. cit.*); an assertion which sufficiently indicates that in all probability some, at least, of these pains arose from pleuritic effusions, limited, perhaps, to one portion of the pleural cavity. Many of the systematic authors of the sixteenth century recognise very distinctly the pleuritis notha, which from that period has, under one name or other, a distinct place in all nosologies.‡

* Acut. Morb., II. 17, *initio*.

† Galen, De Locis Affectis, Lib. V. cap. 3. See also Paulus Ægineta, III. 33; and Aetius Tetrabibl II. Serm. IV. chap. 28—"Of the disease which is apt to be mistaken for pleuritis." The disease thus designated is probably one of the originals of the more modern pleuritis notha. How Dr Badham could construe this into a description of acute bronchitis is to me inexplicable. It was manifestly an acutely painful disease, closely simulating pleuritic inflammation, and produced, according to the author, by crude humours arising from the food. There is no mention of its being connected with catarrh, unless an allusion to the passage of the humours to the pleura and lung be so interpreted, but such an interpretation is unnecessary, and is certainly not in accordance with the general scope of the description, which, however, I admit, it is difficult to identify. I shall have more to say hereafter as to the ancient prototypes of the modern bronchitis. Oribasius, Trallian, Actuarius, Scribonius Largus, and Psellus (this last I know only through a reference by Dr. Adams in Paul. Ægin., vol. i. p 499), all refer, more or less distinctly, to pains of the side which are not pleurisy. I presume they copy after Galen, as do the Arabians. Van Swieten (Aph. 875) finds some slight hints of the same kind in Hippocrates; they are not very definite, and are extremely brief. Celsus also (IV. 6) gives merely a hint as to the existence of non-pleuritic pains.

‡ There are also some descriptions of isolated cases,—e.g., Forestus Obs. et Curat. Med., Lib. XVI. Obs 42, 43—"De pleuritide non exquisitâ, sed mendosâ et spuriâ dictâ." But all the narratives and general descriptions are very much after Galen. Rondoletius, however, distinguishes himself by confounding the treatment while recognising the symptomatic distinctions, (pleuritis vera et non vera). Nicolaus Piso (De Cogn. et Cur. Morbis, II. 8,) has it *pleuritis notha*; Plater (Praxis Med. III. 10), *pleuritis falsa*; Lommius (whose elegant little synopsis contains the very marrow of the Greeks) describes it once more (Med. Obs., Lib. II.) as *pleuritis notha*; in Sydenham it disappears altogether, to reappear in the often-quoted but truly unprofitable work of Verna, (Princeps Morborum Acutorum Pleuritis, chap. 1, 17,) first under the old designation, and then under a new one, which may be given in the author's own words:—"Pleuritis aut *propria*; aut *minus propria*, aut *impropria*. Propria solius pleuræ inflammatio est. Minus propria est vera thoracis partium, aut exterarum inflammatio . . . . ;

But if pain be a symptom common in most cases to the ancient and modern pleurisy, it is by no means of the same significance in the two forms of disease. In the pleuritis vera it would not be difficult to show, by the concurrent testimony of practical writers, that pain and fever in conjunction were the really *guiding* symptoms by which the intensity of the disease was in a great degree measured, and the practice determined. Thus the practice of bloodletting was reserved, in the early times of the art, for the cases of extreme pain, or of pain not relieved by other means. The persistence of pain after expectoration, and after remedial measures, was looked upon as most unfavourable;* and notwithstanding the consideration which the favourable or unfavourable character of the expectoration received from Hippocrates, we find him submitting all these grounds of prognosis to one very simple test—viz., "All expectoration which fails to remove pain is bad; that which removes it, on the contrary, is best of all."† And so with other authors the pain is, to a great extent, the essential feature of the disease; and its removal is at once the indispensable condition, and one of the most important evidences of approaching amendment. It can scarcely be necessary to point out in detail how much all this is removed from the practice of the moderns, to whom pain has become a symptom of very secondary consideration. "The greater part of the symptoms which are supposed to be distinctive of pleurisy," says Dr. Williams, "depend on a much exalted sensibility of the pleura, which is *by no means a necessary accompaniment of its inflammation*. . . . On the other hand, the physical signs in the greater number of cases are very unequivocal; and although they by no means speak of the degree or extent of the inflammation, they seldom fail to announce its presence, and they pretty accurately measure *its most serious concomitant, the effusion*."‡ Statements of this kind from other authors of equal repute will occur to every one.

I shall not dwell in detail upon the other, and, as the ancients would consider them, the subordinate symptoms of pleuritis. It is well-known to those who have given any consideration to the Greek medicine, how much attention was paid to every kind of uneasy sensation and abnormal evacuation; the cough, the dyspnœa, the febrile heat, the thirst, the occasional restlessness and delirium, the unfavourable characters of the urine and alvine discharges, as occurring in pleuritis, had therefore undergone, by the excellent observers of that epoch, a very full examination and discussion. The eminently considerate and practical spirit of these observers is nowhere more clearly shown than in their appreciation of the value of each of these symptoms, and of the greatly increased significance which they acquired when in combination. The idea of pleuritis included nearly the whole of them as essential parts of its definition: and nothing was further from the spirit of the Hippocratic medicine than the exclusive leaning upon a single pathognomonic sign. I have already noticed that the only personal narrative of pleurisy left

impropria tandem est, in quâ revera nullius partis pectoris, aut cæterorum ventrium inflammatio adsit," &c. This division, he says, he uses "brevitatis causâ, et *claritate*" (sic); yet all his arguments for the localization of these affections are *in nubibus!* In the Nosology of Sauvages, and in most works since his time, the *pleuritis notha* appears as *pleurodyne*, or *pleurodynia*.

* Hippocrat. Coac. Prenot. 394. † Idem, 391; and in the Prognostics.

‡ Diseases of the Chest, third edition, p. 106.

us by Hippocrates does not bear any inscription to distinguish it as such; for it is everywhere apparent that this great man, with a truly lofty view of his art, preferred real knowledge to verbal distinctions, and the unwearied study of individual cases of disease to arbitrary refinements in their nomenclature.* Aretæus also takes broad and clear views of the disease under consideration, resting its distinction not on any single symptom, but on a combination of phenomena, which, he remarks, "must harmonize and conspire together, and must arise from a single (exciting) cause; for if they arise at random from a variety of causes, even if they happen to appear at once, the disease is not called *pleuritis*."† The whole ancient practice which has come down to us, and all the better part also of the modern practice in pleurisy, is regulated by similar principles; and it is most important to keep in view that the heroic remedies in thoracic inflammations, whose use and abuse have been the subject of so much controversy, were never applied by the Greeks to anything short of *an acute disease, suddenly arising in a previously healthy subject, and accompanied by great functional oppression, with extreme distress and fever.* If we would always keep this fact steadily in view, we should be saved from many extravagancies which beset the medicine of the present day. We cling to the therapeutical traditions of the past, and rely on an assumed universal experience, while we neglect the studies necessary to make our practice consistent with that of our predecessors, and strike blindly, with their heaviest weapons, at enemies whose very existence was scarcely known to them. In improving diagnosis, we have made a chaos of nosology. What wonder if rash innovators and pedantic obstructives, misled by names, fall into an equally vicious routine? If, on the one hand, the wise are scandalized, and the simple perverted, by practices for which the authority of all antiquity is claimed, but at which common sense revolts? If, on the other, the charlatan and the sceptic simultaneously arrive at the idea, that infinitesimals and extract of grass are better and safer remedies for acute diseases than all that have descended to us from our fathers? The reader of the preceding pages, and of the illustrations of modern practice which I have discussed elsewhere in reference to *pneumonia*, will, I trust, have little difficulty in detecting the misapprehensions which lie at the root of such vagaries.‡ They have a place in the history of opinion, and will pass into the limbo of its vanities, though not without a useful result, if they shall lead us, by their very extravagance, to consolidate more care-

* For his reproach of the Cnidian School on this point, see the admirable introduction to the book Περὶ διαίτης ὀξέων, de victus ratione in morbis acutis.

† Aretæus, Acut. Morb. Causis, &c., I. 10.

‡ In the course of these investigations I have made notes of a considerable amount of evidence bearing on the ancient and modern use of bloodletting, which tends to show that the general experience of that remedy is very far from justifying either the practice of many modern physicians, in refraining from it altogether, or the mad caprices of Botal in the sixteenth century, and Bouillaud in the nineteenth, which, it must be confessed, have proved but too seductive to certain minds. I refrain from pursuing, for the present, this argument, which, however, I hope to overtake at a future time. Meantime, the reader who is anxious to pursue this train of thought, cannot do better than peruse a most suggestive clinical lecture by Dr. Alison, in the Monthly Journal, vol xv. p. 493; in which, besides many most valuable considerations on various therapeutical fallacies, the peculiarities of the modern nomenclature of chest diseases, and its misleading influence, are indicated with a clearness of which there is no other example, so far as I know, in our literature.

fully the structure of our knowledge. It is not yet too late to retrace the lost footprints, and to re-establish the old boundaries, which enabled us to reconcile our experience with that of many great physicians, whose names we still hold in regard.

I have endeavoured in the preceding remarks to show that the ancient use of the term *pleuritis* confined its application to severe, and acute, and transient forms of painful and febrile chest-affections. Pleuritis represented essentially an acute fever, with superadded local symptoms; and no case of what we now call *chronic*, or even *subacute*, still less *latent*, pleurisy, could by possibility have been included under that term, or been the subject of the treatment addressed, as a rule, to pleuritis. It follows that the ancient idea of pleuritis, though theoretically and anatomically ill-defined, was in relation to practice (i.e., to prognosis and treatment) far more simple, clear, and, as it were, homogeneous than ours. It was not without reason that the ancient physicians maintained this disease to be essentially *the same* (in relation to treatment), from whatever exciting cause it arose; whether from dissipation in food or drink, from venereal abuses, from violent exercise, or from external injury;* for it comprised a series of cases practically of the same species, far more so, indeed, than those of pneumonia, selected by Louis for comparison in relation to the effects of bloodletting; although we have seen† that Louis proceeded with a degree of caution and exclusiveness which distinguish his researches from those of most other modern authors, singling out the really severe and acute cases, and thereby obtaining results which afford an easy triumph to homœopathic and other statistical medicasters.

It would, however, be a great error to suppose that the modern idea of pleurisy could be reconciled with the ancient by merely cutting off from the former the chronic, subacute, and latent cases. This process would, indeed, reduce the field occupied by the pleuritis to proportions very inconsistent with the term *princeps morborum acutorum*, applied to it by Verna. I have already‡ alluded to the fact that pneumonia occupies in most of our modern therapeutical disquisitions, the position assigned by the Greeks to pleuritis. I must now endeavour to show, what I believe to be beyond all question, the fact *that the pleuritis of all the authors preceding Galen, and indeed of the entire world of medicine up to the sixteenth century, included (besides the comparatively few cases of acute pleuritic effusion) nearly the whole of the more acute types of the modern pneumonia.*

The proof of this proposition cannot of course be looked for in morbid anatomy; for, although the Greeks formed some shrewd guesses (possibly backed by a few imperfect observations) as to the seats of internal disease, it is evident that they do not (with the exception, perhaps, of Galen and his followers) pretend to accurate and definitive information as to the condition of the diseased parts. Still less can we look for the diagnostic characters in which we are now accustomed to place our chief reliance in distinguishing the inflammations of the chest. We must, therefore, have recourse to the more obvious symptoms and signs, and to the general description of pleuritis and peripneumonia.

* "Una est atque eadem passio, ex quâlibet veniens causâ, quæ unâ atque eâdem indigeat curatione."—Cælius Aurel., Acut. Morb. II. 13.

† British and Foreign Medico-Chirurgical Review, No. 25, p. 213. ‡ Ibid., pp. 208—9.

Now, with regard to the first, it is to be observed that the *pleuritis* was a disease of great frequency and fatality; that it was almost invariably attended by expectoration from an early period, the contrary cases being exceptional;* and that the expectoration which constituted its usual critical evacuation was precisely similar in its character, in most cases, to that which is now regarded as almost pathognomonic of acute inflammation of the lung. It appears to me that these three points are almost conclusive as to the true place in the nosology of the ancient pleuritis, and that they show to how small an extent it corresponded with the *acute pleuritic effusion* of the moderns. For there can be no question that the latter is, comparatively, a rare disease; that its termination directly by death is still more rare;† and that expectoration can by no means be regarded as one of its proper symptoms, or critical phenomena. Add to this, that the operation of paracentesis, which was not unfrequently performed by the ancients for the chronic disease, *empyema*, is scarcely ever mentioned in connexion with pleuritis.‡

It appears to me very certain, from these and other considerations, that the differences between the ancient and modern pleuritis are not those of degree only (as formerly discussed); and that these *two diseases* (for so we may now call them) are not merely symptomatically, but anatomically distinct, in a large proportion of cases. In other words, that a very great majority of the cases of Greek pleuritis were the pathological equivalents of the modern pneumonia; and that the small proportion of cases of acute pleuritic effusion constituted an exceptional variety of the pleuritis, which was commonly ascribed, after the fashion of the humoral pathology, to defective coction of the morbid matter. It remains that I should illustrate the true relation of the pleuritis to the peripneumonia, by placing before the reader a graphic picture of each. I shall extract its elements from Aretæus and Cælius Aurelianus,§ adopting chiefly the expressions of the latter.

* Such cases are said by Galen to be called ἀπέπτους πλευρίτιδας, *unconcocted* pleurisies. He adds that they are either quickly fatal, or are resolved *by a slow process* (χρόνῳ πλείονι λυομένας). This last expression must be understood as opposed to the *crisis*, which was, in the opinion of the Greeks, the natural termination of an acute febrile disease, such as the pleurisy, in its ordinary form, undoubtedly was. This description of the unconcocted pleurisies agrees very well with the characters of pleuritic effusion, as we now know it.—Galen, De Locis Affectis, Lib. V. cap 3.

† Dr. Walshe says, on this point—"I have neither myself lost a patient from pure primary idiopathic pleurisy, with or without effusion, nor known of an occurrence of the kind in the practice of others. And although, where chronic disease, either of the lungs or of other organs, pre-existed, death is a more common result, it is still an unusual one." My own experience of this disease has been somewhat less happy than that of Dr. Walshe; but I can readily accept this testimony, which concurs with that of Louis. Diseases of the Lungs and Heart, p. 366.

‡ Galen mentions, as we have seen in a former passage (p. 245), that the non-pleuritic pains of the side sometimes result in an abscess opening externally. He does not, however, allude to any such occurrence in the true pleurisy, whether of the ordinary kind, or of the unconcocted variety, which, in this very passage, he takes much trouble to distinguish from the non-pleuritic pains. In true pleurisy, he says there is cough; in the false pleurisy, none. It is plain that Galen here falls into his usual error of over-refinement.

§ I choose these authors in preference to either Hippocrates or Galen, because at the epoch of the Hippocratic writings, the definitions of disease had hardly assumed form and consistence; whereas, in Galen, they are so much stereotyped, as it were, and mixed up with anatomical and physiological hypotheses, as to carry but little of the impression of reality to the mind. Cælius Aurelianus, notwithstanding his crabbed and corrupt Latinity, is unquestionably one of the most valuable and instructive of all the authors of antiquity. I know not how so wretched a linguist came to be possessed of such clear ideas; unless, indeed, we sup-

The pleuritic passion, according to these authors, is marked by acute fever, with cough and severe pain of the side, extending to the root of the neck and to the scapula: sometimes also affecting the arm, breast, and ilium. The respiration is difficult; the cough sometimes dry,* but commonly with liquid expectoration, at first frothy, afterwards sanguinolent, also bilious (yellow), and then sanious. The patients are also affected with difficulty of lying on *that* side,† and when they turn on the opposite side they experience pain, from a sensation as if the swelled viscera were dragged out of position by their weight. Various incidental symptoms are then described; watchfulness, dry rough tongue, cold extremities, suffusion of the eyes and countenance, vomiting, delirium, &c. The pulse‡ is large, rapid, hard, strong, and fluctuating. There is sometimes a rattling in the throat, sometimes a noise resounding or whistling internally on that side which suffers.§ In the transition of pleuritis into peripneumonia|| (a frequent and fatal change, according to all the ancient authors), the pain alone is relieved; the other signs become all worse, the pulse is feeble or absent, and the decubitus is supine. When a *vomica* supervenes on pleurisy, the pain becomes fixed in one place, and is diminished; there is dry cough; the respiration is difficult, and the typical rigor or tremor (of hectic fever?) comes on, the pulse being large. When empyema or vomica occurs as an independent affection, there are shiverings,¶ and pungent pain; and as the pus is at first collected in the side, the occurrence of expectoration is secondary; whereas, in pleuritics, the fever is severe from the first, and the sputa are preternatural, and of various kinds.**

pose, with several authorities, that he was a mere translator. It indicates, however, a very marvellous preference for style over matter, on the part of mankind, that the elegantly-written, but common-place compilation of Celsus should have run through hundreds of editions, while these truly instructive and original writings have found only one careful editor, and no translator, so far as I am aware, in modern times.

* "Tussicula aliquibus arida, frequenter tamen cum liquidis excrementis, et primo spumosis, dehinc sanguinolentis, ita fellosis, ac inde saniosis."—Cælius Aurel., A. M. II. 14. He afterwards describes at length these and other varieties of the pleuritic expectoration, and says, that the order of their ejection is often changed (mutato sæpe ordine suæ egestionis). They are, besides, *not homogeneous* (inæqualia). According to Aretæus, dry cough, sputa difficult to be brought up, composed of phlegm, or of matters like bile (χολῶδες), or largely mixed with blood (δίαιμον κατακορέως), or yellowish (ὑπόξανθον); and not keeping this order, but promiscuously appearing and disappearing. The worst of all, however, he says, is when the blood-mixed sputum suddenly disappears.—Aret. de Morb. Acut. Causis, &c., I. 10.

† "*Id latus.*" There is evidently some confusion here; for Aretæus, who has the same ideas of the dragging of the viscera, says, that on the inflamed side the decubitus is well borne, because the membrane (of the pleura) settles into its proper place (ἐν ἕδρῃ γὰρ ἕζει τῇ ἑωυτέου ὁ ὑμήν), while, by reclining on the opposite side, pain is excited. See the chapters already cited. It is now well known that the phenomena of the decubitus in the early stages of pleurisy are not to be depended on.

‡ Aretæus does not mention the pulse. Galen describes it as *small*, and indicating a hard and tense artery.—De Locis Affectis, V. 3. Modern physicians (many of them, perhaps, without knowing it) have adopted too many of Galen's intricate and fantastic descriptions of pulses, in which, as usual, his imagination runs a long way in advance of the facts; as any one may see in his books De Pulsibus.

§ "Gutturis stridor, vel sonitus interius resonans, aut sibilans, *in eâ parte quæ patitur*." (Loc. cit.) Laennec, whose reading was commonly as extensive and careful as his personal studies of disease, seems to have missed this passage; otherwise he would surely have put it beside the one which he quotes from Hippocrates—de Morbis—as showing the origin of auscultation among the ancients. There is a somewhat similar passage in the Prognostics, in which Hippocrates describes the boiling of matters in the air passages. See further on, p. 253, note.

|| Cælius Aurel., loc. cit., c. 15. ¶ Loc. cit., c. 17.

** According to Aretæus, loc. cit., if pleuritis is not resolved in three weeks, it changes to

So much for the pleuritis, its consequences, and its diagnosis. Let the reader compare with this the description of the peripneumonia, as extracted from the same authorities.

Peripneumonia is accompanied by acute fever, a sense of weight in the thorax, and of labour in effecting its movements;* the decubitus is supine, or a little raised; sometimes the sitting posture is preferred. The face is florid, especially the cheeks; the veins of the temples and neck are dilated; the eyes are bright and full (pinguis†). Moreover, the breathing is accelerated, and there is cough, with sanguinolent and bilious (yellow) or fuliginous (?) sputa, *more tawny and frothy than those of pleuritis.*‡ The pulse is vehement and rapid (various other general symptoms are here described). When the disease becomes worse, the thorax is rather protuberant,§ and the breathing is distressingly difficult, accompanied by *a certain vehement and harsh whistling;* at last the pulse is latent or creeping, such as the Greeks call μυρμιζοντα, and a sounding noise is heard in the breast, which they call *rhogmon.* The circumstances which chiefly distinguish the peripneumonia are these,—acute fever, rapid and difficult breathing, cough, expectoration of various kinds, *oppression* (gravatio) *without any pain, or with slight pain only,* and a sense of suffocation.

That the reader may be the better enabled to contrast the ancient peripneumonia with the pleuritis, and that he may be likewise led to bring the descriptions of both diseases into comparison with the modern affections bearing the same or similar names, I shall place in a tabular form the symptoms of these affections, together with some reference to the opinions maintained in respect to their nature and seat.

PLEURITIS.	PERIPNEUMONIA.
Acute fever.	*Acute fever.*
Cough.	*Cough.*
Pain of one side, severe (vehemens), pungent, extending to neck and scapula, &c.	*Pain* absent, or slight, not localized; sense of weight in the thorax (gravedo, gravatio).
Respiration impeded and accelerated (δύσπνοια). (Galen and others distinctly describe the thoracic movements as checked by the pain.)	*Respiration* rapid, accomplished by means of laboured movements of thorax.
Expectoration sometimes absent (but this was exceptional, and more characteristic of *empyema* or internal suppuration); commonly charged with blood, or yellow; sometimes mucous, and varying during the progress of the disease.	*Expectoration* never absent, not differing much in appearance from that of pleuritis; but, on the whole, more yellow and frothy.

empyema. (I have already noticed the ideas of Hippocrates on this point, see *ante*, p. 244.) The great danger is, that the lung, being a warm and rare organ, may attract all the matters to itself: whence follows either rapid death by suffocation, or an internal suppuration.

* The text of Cælius is here very corrupt, but there can be no doubt of the general meaning. "Febres acutæ, gravedo thoracis, et sensus laborantium quâdam difficultate laterum, atque medium papularum."—Acut. Morb., Lib. II. c. 26. All other authors agree as to these facts.

† Compare Aret., Acut. Morb Caus II. 1. The references to the "fat" appearance of the eyes, and the bluntness of the nose, are a little puzzling. I think it better to leave questionable and trivial details out of view, and only to give the general sense. The curious reader may refer to the originals.

‡ "Sanguinolenta, atque fellea, vel fumosa jactans sputa, et in comparatione pleuriticorum fulviora, vel spumosiora."—Cæl. Aur. loc. cit. We know something of fuliginous or carbonaceous sputa now-a-days, but I question whether these are here meant.

§ Possibly this idea is connected with the supposed swelling of the lung in peripneumonia, a fancy which continued to prevail down to the time of Laennec, and which that observer did not fail to notice and to correct.

Decubitus lateral.	*Decubitus* dorsal or suberect.
Pulse large, hard, and strong.	*Pulse* at first strong and rapid, afterwards small and indistinct
Countenance injected and suffused.	*Countenance* congested, with swelling of temporal or jugular veins.
Abnormal sounds occasionally heard on the side affected.	*Abnormal sounds*, not localized, and of a loud and harsh character.
Disease believed to be unilateral, and to have its seat in or about the costal pleura,* and the neighbouring parts. (Some, however, maintained that it was in the lung itself.)	*Disease believed to be* bilateral and general, and to have its seat in or about the lung,† which, from its anatomical structure, was believed to be incapable of feeling pain. Hence the idea, "plus periculi quam doloris," as applied to peripneumonia.

The preceding descriptions, and their analysis, will, I believe, be found sufficient to sustain the conclusion which I have already placed before the reader by anticipation—viz., that the *pleuritis* of the ancients included, besides the more acute forms of pleuritic inflammation, the greater part of the cases of modern *acute pneumonia.* It may, however, be desirable still farther to illustrate this proposition, and to lead the way to another, by a few considerations tending to show in detail, not only that the description of the ancient pleuritis corresponds, for the most part, accurately with that of the modern pneumonia; but that the peripneumonia, as described by the Greeks, does not do so; and that, on the other hand, the latter does correspond, in general, with another form of disease.

Let the reader, then, consider, for a moment, the severe pungent *pain* which was the most striking attribute of the Greek pleuritis, and contrast this affection with the peripneumonia, in which pain was either absent, or slight and non-localized. Let any careful observer, then, compare these two diseases with the pneumonia of the moderns, in its really acute forms (excluding, of course, the cases of typhoid and catarrhal pneumonia, of which I gave an account in a former paper), and let him say which of the two, peripneumonia or pleuritis, most nearly represents

* Called ὑπεζωκὼς (ὑμήν), the encircling membrane, so called, says Galen, because it encircles inwardly the whole side. Its relation to the ribs and intercostal muscles was pretty well understood; but it is important to recollect that the *pulmonary* pleura was totally unknown till a comparatively recent period The humours which produced the pleurisy were therefore supposed to be collected between the costal pleura and the ribs; and the pain was due to the compression which the parts sustained, as well as to the seat of the disease being a membrane part, which, like all the other membranous parts, was considered to be derived from the membranes surrounding the brain, and to be amply furnished with nerves or fibres. The lung, on the contrary, being scantily provided with *nerves*, was wholly insensible; and this argument was conceived to fix the lung as the seat of the peripneumonia, as well as to form an answer to those of the ancients who alleged it to be the part involved in pleurisy. The controversies on this point, beginning as they did in the earliest times, were not much nearer a settlement at the time of Morgagni. We shall see hereafter how much confusion they have introduced into the nomenclature, and even the descriptions of authors. Meantime, compare Galen, De Anatomicis Administrationibus, c. 2; Idem, De Locis Affectis, II. 3, and V. 3; Aretæus, De Acut. Morb Causis, &c., II. 1; Cælius Aurelianus, Lib. II. c. 16 and 28.

† The chapters of Cælius Aurelianus referred to in the preceding note, give a curious and most interesting view of the anatomical speculations of the ancients, as to the special seat both of pneumonia and pleurisy. With regard to the peripneumonia, it is evident that the veins, arteries, and bronchi of the lung had all fallen under suspicion, and it does not appear that anything more definite was known up to the beginning of the sixteenth century. The term *peripneumonia* has puzzled authors and etymologists not a little; some of them supposing that περὶ here means *around*. Laennec repudiates this, and argues, with some reason, that περὶ is merely an intensive preposition; as though the meaning were, the *general affection*, or *very grave* inflammation of the lung. On the other hand, Galen remarks that, according to some persons, the lung itself is not susceptible of inflammation; but that the adjoining parts being in this condition give rise to *the inflammation about the lung*, περὶ πνεύμονα.—Galen, Isagoge, seu Medicus.

the acute inflammation of the lung as known to him. Is the modern pneumonia a painless disease in a large proportion of cases when the above deductions are made? I scarcely anticipate much difference of opinion on this point; but lest my own statements be regarded as biassed, I shall place below the answer of Grisolle,* undoubtedly the most exact of modern observers of this disease, which show that in primitive acute pneumonia the absence of localized latent pain is a rare occurrence, and that the disease now called pneumonia never could have led the Greeks into the belief that its essential seat was in the "insensible lung" (ὁ πνευμων αναίσθητος).

What, then, was this *painless*, or slightly painful, affection of the lung, more dangerous than painful, accompanied by acute fever, cough, various expectoration, difficult and laborious respiration, orthopnœa, and all the signs of impending asphyxia? What was that disease which, when it supervened on pleuritis (pleuro-pneumonia), was almost invariably fatal, because it superadded to the *local* a severe *general* affection of the lung? What was the disease in which a "vehement and harsh whistling" was habitually heard, and in which, on the suppression of expectoration, the lung was wont to "fill with matters which boil in the air passages?"† Such was the ancient *peripneumonia*. It is to me evident that such, too, is the modern *acute bronchitis*.

In acute bronchitis the respiration is not short and hurried, but, even when it is much more frequent than natural, it appears long-drawn and laboured. This was precisely the distinction between peripneumonia and pleuritis.

In acute bronchitis, the expectoration differs from that of pneumonia and pleurisy chiefly in being more yellow and frothy. This was also a distinction of the peripneumonia.

In acute bronchitis, the decubitus is dorsal or suberect, not lateral. This was also the case in the peripneumonia.

In acute bronchitis, a strong and rapid pulse is quickly reduced, by the advance of the disease, to a fluttering or "latent" one. This character was also conceived to mark the pulse of the peripneumonia.

In acute bronchitis, the veins of the neck and face become not unfrequently swollen, especially when the disease occurs in connexion with emphysema and disease of heart. This symptom was also insisted on in the peripneumonia, which was, moreover, the most frequent and best established fatal termination of almost all other thoracic diseases, and especially of catarrh.

Finally, in acute bronchitis, a sense of weight or oppression in the thorax takes the place of positive pain, and is perhaps the most marked abnormal sensation. The description of the peripneumonia here, again, is exactly in accordance with that of the modern disease.

On these grounds, as well as on others which will probably occur to the readers of ancient descriptions, but which if detailed here might

* "Une douleur plus ou moins vive *siégeant dans un des côtés de la poitrine* est un symptôme qui accompagne *presque constamment* la pneumonie. Il résulte, en effet, de mes observations que sur 301 malades affectés d'inflammation du poumon, la douleur de côté n'a manqué que chez 29, ou chez un dixième environ." A further statement shows that the pain was most accurately localized.—Grisolle, De la Pneumonie, p. 198.

† See Hippocrates, Prognostics, ἀλλὰ πλήρης ἐὼν (ὁ πλευμων) ζέῃ ἐν τῇ φάρυγγι.

seem over-refined, I am disposed to identify the *peripneumonia* of the Greeks with *acute bronchitis* far more than with *pneumonia.* Yet I would by no means be understood to deny that a certain number of cases of inflammation of the lung, or of broncho-pneumonia, fell under the designation of peripneumonia; in the same way as a certain proportion of cases of pleuritic effusion without pneumonia fell under the designation of pleuritis. I believe that both these results were inevitable, and that they actually took place. In fact, the general scope of the Hippocratic writings would seem to imply (what, indeed, is undeniably the fact), that the localization and distinction of the different acute diseases of the chest was by no means so easy in practice as it was in nomenclature and in theory. How, indeed, could it be otherwise at that period? Are these distinctions absolutely clear, even at the present day? I venture to believe that no one familiar with disease from personal observation, and particularly with the phenomena which I have elsewhere described as interfering with the stethoscopic diagnosis of pneumonia, will hesitate to admit the negative.

Yet it is of extreme importance, in studying the past history and progress of the medical art, to possess a nosological index, however imperfect, to the works of those who have gone before us in observation and in theory; and who have left us a legacy of opinion, despised by some, neglected and forgotten by others, but clinging with not less tenacity to those who treat it with professed indifference and contempt, than to those who have endeavoured to appreciate and understand it. On this ground I venture to propose the following interpretations of the ancient nomenclature of diseases of the respiration, submitting them, not without much diffidence, but yet with a strong feeling of conviction, to the verdict of those more accustomed than I am to such inquiries.

1. That for the περιπνευμονία, or περιπλευμονία of the Greeks, we may usually read *acute bronchitis*, or *broncho-pneumonia;* and in every case an extremely acute febrile disease, with little pain, tending to rapid suffocation and prostration of the vital powers.

2. That for the πλευρῖτις we may read *pleuro-pneumonia*, or more rarely *pleurisy*, excluding the chronic, trivial, and protracted cases of both; a highly acute *localized* febrile disease, attended by hard firm pulse throughout, but sometimes passing into the περιπνευμονία, and then assuming its characters.

3. That in the descriptions of ἐμπύημα, and in the case of the corresponding expression ἔμπυοι, we should have in view not merely the disease called empyema, but a much wider range of sub-acute and chronic affections—viz., all cases of acute disease of the lung and pleura indiscriminately when protracted beyond three, or, at most, six weeks; and in addition to these, all diseases commencing as chronic affections, and ending in purulent discharge, whether by expectoration, external opening, or otherwise; whether in the chest or in the abdomen.

One further consideration appears requisite in order to illustrate completely the above propositions, and the nosological revolutions connected with them. The history of modern inquiry ought to afford some clear explanation of the modifications which have taken place in the ideas of

the classical period as to peripneumonia and pleuritis. The links between the nomenclature of the past and that of the present day deserve investigation. Fortunately, the materials for this inquiry still exist; its results, too, are both curious and interesting.

The close of the fifteenth century brought with it in medicine, as in other departments of human knowledge, a revival of the classical, and especially the Greek models, in opposition to the mediæval commentators and encyclopedists. Almost simultaneously, the spirit of observation began to awake, clinical facts were recorded with something like independence, and anatomy, which had been to some extent cultivated and reformed by Mundinus and his successors, was applied to the discovery of the seats of disease, in regard to which the Greeks had made few, if any, direct observations. It is impossible to deny that in the glimmering twilight of this period pathological observations were recorded which have not been discredited by the progress of science; and if the sixteenth century failed to produce its Morgagni as well as its Vesalius, it was not so much from the want of a scientific spirit at its commencement, as from the erratic fanaticism of the Paracelsists, whose clamorous folly succeeded to a considerable extent in withdrawing attention from the incipient science of morbid anatomy, and, indeed, well-nigh supplanted clinical observation by the angry disputes of the chemists and Galenists. It was not till the middle of the seventeenth century that the storm blew over; many of the earlier observers had then been almost forgotten; and had it not been for the laborious collections of Schenck and Bonet, and the well-directed efforts of a few distinguished men, whose truthfulness and simplicity of character withdrew them from the theatre of this disastrous and turbid controversy, medicine would have fallen back again into worse than mediæval darkness. Perhaps, in our somewhat exaggerated, but still well-founded, admiration for Sydenham as "the English Hippocrates," we have too hastily set aside some of his predecessors. Plater, at least, deserved more attention than he has received; the same may be said of not a few still earlier leaders in the path of original observation. The present subject affords an illustration of this remark.

All the systematic nosologists, from Sauvages to Cullen—and, indeed, all the writers of the eighteenth century, not excepting even the learned and generally accurate Morgagni—ascribe to Sydenham the original description of the *peripneumonia notha.* All of them discuss its nature, as if it were a new disease, or, at least, one greatly neglected by the older physicians.* To judge, indeed, from Sydenham's own descriptions, it can scarcely be supposed that he intended to introduce the *peripneumonia notha* to the world as a member of the *nova febrium cohors.* He expressly says that it was of annual occurrence: "Hyeme ingruente, at sæpius sub ejusdem exitum, vereque adhuc nascente, *quotannis emergit* febris symptomatis peripneumonicis haud paucis conspicua."† Besides, his classification of it, with pleuritis and rheumatism, among the *intercurrent* fevers, seems to show that he regarded it as one of the permanent denizens of the English soil, at least in his own age. Still, the habitually terse and

* See Van Swieten, Commentar. in Boerhaavii Aph. 867; Lieutand, Precis de la Médecine Pratique, Livre I.; Cullen, First Lines, Part I., Book II., chap. 7.

† Sydenham, Observationes, VI. 4.

dogmatic style of this author, and the circumstance of his using, without explanation, a term not familiar to the readers of the classical literature, contributed to diffuse the impression, that Sydenham either invented this term, or gave to it a new significance by attaching it to a hitherto neglected disease.

The difficulties which have been felt and expressed by authors in relation to the *peripneumonia notha* of Sydenham show very clearly that the term, unless applied to a new and rare disease (which it was not), is an unnecessary refinement on the ancient nomenclature. In the first place, it must be remarked, that Sydenham himself, in order to find room for this "fever *distinguished by most of the symptoms of peripneumonia,*" absolutely removes from the nosology the peripneumonia itself; of which he merely says, that it is "of the same nature as pleurisy, and only differs from it in affecting the lungs more generally."* Boerhaave, on the contrary, describes both the true and false peripneumonia; but the peculiarities that he ascribes to the latter are almost entirely theoretical, relating, as they do, more to its cause than to its symptoms. It is impossible to read the portion of Morgagni's twenty-first letter† which relates to this subject without feeling that the identification of this disease was by no means an easy or satisfactory task to him; and the same remark will apply to Lieutaud. Finally, Cullen adopts Sydenham's distinction; but Cullen also refuses to distinguish the *peripneumonia vera* from the pleurisy. In both these respects he agrees with John Peter Frank.‡

The reader of the preceding pages must have already divined the cause of these embarrassments. The distinction of Sydenham was a distinction without a difference; for his *peripneumonia vera*, which he believed to be identical in nature with pleurisy, was, in fact, the pleuritis of the ancients; and his *peripneumonia notha*, making allowances for theoretical and insignificant differences, was the peripneumonia of the ancients. In other words, it was only *by losing sight of the ancient distinctions between pleurisy and peripneumonia* that the moderns contrived to introduce a new febrile acute disease of the chest. By doing so, they effectually broke the link between the ages, and established a barrier of words between themselves and the Greeks. It was not, however, Sydenham who was directly responsible for this result. A very slight acquaintance with the writings of that great physician and of his predecessors will show that, in regard to nomenclature at least, he was no rash innovator; and that, in this instance, he merely adopted a term which had been current for the greater part of a century.

To show the real origin of this change, we must go back to the clinical observations of the sixteenth century. One of the earliest collective writings in which the actual name "*peripneumonia notha*" occurs is the work of Forestus, who gives it, however, not as his own innovation, but as a term used among his contemporaries:

"Non desunt," he says, "qui in *veram* et *legitimam* et *notham* (peripneumoniam) distinguunt; unde et Fernelius *raram admodum veram peripneumoniam esse asseruit*, cum et tenuis et aëris multaque destillatio è cerebro confertim in pulmones illapsa, interdum ardore præter naturam incenditur, æstum ac febrem inferens; ac *plerique*

* Loc. cit. † De Sedibus et Causis Morb., Epist. XXI. 13, 14.
‡ Frank, De Curandis Hominum Morbis, vol. i. § 183 et seq.

eam affectionem peripneumoniæ nomine designant, in quâ et tussi et spiritus difficultate et lentâ febre sensim conficitur æger sine ulcere, et sine expuitione cruentâ. Hæc si peripneumonia dicitur, ab exquisitâ profecto plurimum tum causâ tum symptomatum magnitudine dissidebit."*

The latter part of this passage is merely a quotation from Fernelius. Now Fernelius, although he protests against the undue extension of the idea of peripneumonia, and asserts that *true idiopathic peripneumonia* (quæ non alium morbum subsequitur, sed ex sese primum duxit originem)† is a rare disease, does not himself employ the term *peripneumonia notha*. On the other hand, this term is employed by Forestus (as we have seen), by Nicolaus Piso,‡ and possibly by other compilers, who either quote or refer to Fernelius as the source of their ideas. Lommius, another contemporary compiler, follows Fernelius very exactly in his 'Observationes Medicinales,' and, like him, leaves this spurious and evidently catarrhal peripneumonia without a name.§ From all which circumstances it is quite clear that the name first acquired currency among the numerous followers of Fernelius, being applied by them to the *more dangerous kinds of catarrh, i. e., to bronchitis and broncho-pneumonia, unattended by extreme fever and bloody expectoration.* This has also been considered, by the more modern writers, to be Sydenham's application of the term. Frank, indeed, describes it as a "catarrhus bronchiorum." Badham identifies it with the modern *sub acute* bronchitis.‖ Sydenham's description, however, refers to a highly acute disease, evidently only one stage removed from the most acute forms of thoracic inflammation, and distinguished by the intensity of its febrile accompaniments, from asthma, and from the *febris hyemalis*, which he describes in a postscript to the treatise, 'De Podagrâ et Hydrope.'

Now, I have endeavoured to show above, that the Greek peripneumonia was distinguished from pleuritis by precisely those characters which separated Sydenham's *peripneumonia notha* from pleuritis or from the true peripneumony, regarded by him as the same disease. It will be observed that both Sydenham and Fernelius come to the conclusion (though on different grounds), that the name *peripneumonia vera* is of very limited application. This was certainly not the Greek idea. There is no hint in any classical writer that the peripneumonia was otherwise than a common disease; and we have seen above that the descriptions of it were by no means wanting either in clearness or in truth to nature, if we may judge by the forms of disease which nature presents to our observation at the present day.

The more closely the details of this subject are examined, the more evident does it become that the distinction between the *true* and the false peripneumony sprang from an entire misapprehension of the idea of peripneumonia, as understood by the ancients. The assertion of Fernelius that it was in his time "very rare," would be utterly incomprehensible (except, indeed, upon the theory that the whole face of nature

* Forestus, Observ. et Curat. Med., Lib. XVI., Obs. 44.

† Fernelius Patholog., Lib. V., chap. 10.

‡ N. Piso, De Cognosc. et Curand Morbis, Lib. II. 3.

§ Lommius, Med. Obs. Liber Secundus. "Non omittendum autem *id vitium pulmonis* est, quod ex tenui, acri, multâque destillatione e capite confectum in hunc ipsum illapsa oritur," etc.

‖ Badham on Bronchitis, 2nd Edition, p. 5.

had changed) were it not for the distinct proofs which I shall presently adduce, that even before the middle of the sixteenth century (and Fernelius's great work was not published till 1554), the very existence of peripneumonia as a separate form of disease had been brought into controversy, in consequence of the progress of anatomical knowledge, and its supposed bearing upon nosological questions. That the name peripneumonia notha, on the other hand, was not a real gain to science, seems to be sufficiently demonstrated by the confusion which followed its introduction; for although the writers after Sydenham for the most part retain the names of both the true and the false peripneumony, they show perpetually that they do not know what to do with them, and that they would willingly suppress either the one or the other. Thus, Hoffman and Cullen confessed, like Sydenham, the inability to distinguish the peripneumonia from the pleuritis; De Haen and Stoll, following Vincent Baron, merged these two names into a compound term, and thus maintained the identity of the diseases; Morgagni and Lieutaud, even while making the same admission, hesitated in adopting the new nomenclature (which they referred to Sydenham and Boerhaave), on the ground that authors were not agreed about the characters and nature of the *peripneumonia notha.* It is unnecessary again to refer to the evidence that more modern writers have misapplied both of the Greek terms, and by identifying bronchitis and broncho-pneumonia with Sydenham's description rather than with the peripneumonia of the Greeks, have crowned the confusion, and lost the key of the whole ancient pathology, as regards thoracic acute disease. In Dr. Badham's work, this misunderstanding is clearly apparent. The author sweeps through ancient and modern literature in search of bronchial inflammation, which he discovers everywhere in odd corners and exceptional passages; leaving the impression that this disease, though "touched upon in various writings on practical medicine," was nearly unknown to the whole ancient world, and scarcely sufficiently known—at least, in its most acute form—up to the date of his own work.

To find the source of the nosological movement by which the peripneumonia of the Greeks was broken up into new elements, we must have recourse to other authors than Fernelius, who, though he embodied in his erudite system of medicine most of the ideas of his time, can scarcely be said to have added largely to them by his own observations. It was in the course of the anatomical investigations of the sixteenth century, to which I have already alluded, that the question was raised as to the independent existence of the peripneumonia. The points thus brought into controversy were of great importance.

In the first place, anatomical investigations taught the physicians of the sixteenth century that the pleuritis of the Greeks was not, as had been supposed, an inflammation especially of the costal pleura, or *membrana succingens;* but that it almost always involved the lung and its covering, and generally, indeed, appeared to have its origin there. The opinion of Galen and the Arabians was not indeed given up without a struggle; and long after this period it continued to be held by Sennert, Diemerbroeck, Riolanus, and others, that the pleura of the ribs only was affected in some cases of pleuritis, and that the lung and its pleura were

secondarily involved.* But the results of unbiassed observation afforded multiplied proofs that in pleurisies the lung and the pleura were commonly simultaneously affected; and Plater, accordingly, with his usual good sense, assumes that the peculiar expectoration of pleuritis, which the Greeks had supposed to be a critical evacuation from the pleura, was in reality an expectoration from the lung itself. He also concludes, as Sydenham afterwards did, that there is no important distinction between the symptoms of inflammation of the pleura and that of the lung.† It is easy to see how the confusion of peripneumonia with pleuritis sprang out of this opinion. . . . . . .

In the second place, the physicians of the sixteenth century learned from anatomy that the peripneumonia of the Greeks was not allied to any single group of morbid appearances; and, in particular, that it could not be regarded as the symptomatic correlative of inflammation of the lung, which, as we have seen, they found to be the most frequent cause of the pleuritis. Thus, while it occasionally happened that those affected with peripneumonia had the lungs inflamed and hepatized, or suppurated (as described by Lælius a Fonte, Dodonæus, and others), cases constantly occurred which were rightly considered to be exceptions to this law. It was undoubtedly the existence of such cases which led to the idea of a false or spurious peripneumony.‡ Nor was it an incorrect observation that the peripneumonia of *Galen* (i.e., an inflammation of the lung having the symptoms of the peripneumonia) was a rare disease; for the true inflammation of the lung is almost always unilateral and attended by pain, whereas the peripneumonia was general and painless.§

To sum up in a few words the principal elements of this protracted inquiry: it will be remembered that the Greeks did not attempt to distinguish more than two severe acute diseases of the respiration—i.e., the painless *Peripneumonia,* resulting, when fatal, in suffocation and rapid prostration; and the acutely painful *Pleuritis,* attended by hard pulse and high fever throughout, but often terminating in peripneumonia. From these two affections, the modern correlatives of which are *Bronchitis* or *Broncho-pneumonia* on the one hand, and *Pleuro-pneumonia* or *Pleurisy* on the other, all the chronic or even subacute, and all the trivial cases, were separated under other names (as Catarrh, Phthisis, Empyema, Pleuritis notha, &c.). The anatomical seat of these diseases raised much controversy, but was never held to be decided, until Galen, with his habitual dogmatism (but with no better information than his predecessors), laid

* The whole of this controversy may be found in Bonetus, Sepulch. Anat., Lib. II. § 4, Obs. 14, 20; and in Morgagni, Epist. XX., XXI.

† Felix Plater, Praxis Medica, Lib. III. 10; and Observationes, Lib. II.—in pectoris dolores.

‡ The following words by Cardan are remarkable, as presenting perhaps the earliest instance on record of a distinctly described bronchial affection. In recording certain cases of epidemic catarrh, he remarks of one of them:—"Dissecuimus illum, invenimusque non esse abscessum; sed omnia vasa pulmonis ex triplici genere, *magis tamen asperæ arteriæ rami* erant pleni sanie simili lacti. . . . Fluxio illa non erat, ut duæ priores; sed *pulmonia mendosa.* . . . . Differt hic morbus a pulmonia, quod abscessus non est."—De Providentiâ ex anni Constitutione, quoted in Schenck, Lib. VI., Obs. 4.

§ The localized character of inflammation of the lung is remarked by Joubertus, De Affect. Thoracis, cap. 7, (see also Schenck, Lib. II., De Peripneum., Obs. 3,)—"In pulmonum *non est necesse totum pulmonem inflammari,* (as Galen and the Arabians had taught); quin sæpissime unus aut alter lobus id patitur; ut infinitorum cadaverum apertio nos docuit."

down what proved to be the law on the subject for the whole middle age.

The earliest cultivators of morbid anatomy proved that Galen was wrong in his ideas; that the pleuritis was not an inflammation of the costal pleura, but of the lung; and that the peripneumonia was not in all, or even in most, cases coincident with an inflammation of the lung. Hence arose the name peripneumonia *notha;* the peripneumonia *vera,* or true inflammatory affection of the lung, being regarded by the best writers of the sixteenth century as but little different from pleuritis, and being likewise treated as indistinguishable from pleurisy by Sydenham, Huxham, Morgagni, De Haen, Stoll, Hoffmann, and Cullen, in more modern times.

Finally, the key of the ancient nosology has been doubly lost by the moderns:—1st, by the description of trivial and chronic diseases under the names which formerly were applied only to serious and very acute ones; 2nd, by the introduction of *anatomical* distinctions and names, without sufficient regard to the *symptomatic* distinctions and names which they supplanted or thrust aside. But as a good and practical nomenclature of diseases ought to be founded on symptomatology, and not on morbid anatomy, it still remains for consideration how far clinical observation corresponds with our present nomenclature. From what we have already seen, it would appear that the practice of physicians has been in some cases endangered, and their diagnosis perplexed, by the present neglected condition of nosological science, which renders it impossible to connect accurately the experience of the past with that of the present day. It ought, therefore, to be carefully considered, whether the same evils cannot be avoided in future.

PART FOURTH.

Chronicle of Medical Science.*

ANNALS OF PHYSIOLOGY.

By Hermann Weber, M.D., Physician to the German Hospital.

I. Food and Digestion.

1. *Succi Gastrici Humani Indoles Physica et Chemica.* By Otto de Grunewald. (Dissert Dorpat, 1853.)
2. *Succi Gastrici Humani vis Digestiva.* By E. De Schroeder. (Dissert. Dorpat, 1853.)
3. *De Succo Pancreatico.* By Sigismund Kroeger. (Dissert. Dorpat, 1854.)
4. *Some Remarks on the Gases of Digestion of Horses.* By Professor Valentin. (Vierordt's Archiv, xiii. 3, pp. 356, ss. 1854.)

1, 2. Grunewald and Schroeder made their observations on a peasant woman affected with fistula of the stomach. The woman was otherwise healthy,

* We have been compelled to defer, till future numbers, Reports on Chemistry, and on Materia Medica, and to shorten the Reports on Medicine and on Midwifery. We do not undertake to notice all new papers in the Chronicle, but trust not to overlook any important article. As less accessible to most of our readers than the British periodicals, the Foreign journals are chiefly reported. During the months of September, October, and November, we received the following Foreign journals.

GERMAN.

1. Archiv für Anatomie, &c., von J. Müller. 1853, Heft 6.
2. Archiv für Phys. Heilkunde, von Vierordt. 1854, Heft 3.
3. Archiv für Pathol. Anat., von Virchow. Band vii. Heft 1.
4. Archiv für Chemie und Mikroscopie, von Heller. Band ii. Heft 10—12.
5. Zeitschrift der K. K. Gesell. der Aertze zu Wien, von Hebra. July, August, September.
6. Zeitschrift (Henke's) für die Staatsarzneikunde, von Behrend. 1854, Heft 3.
7. Zeitschrift für Psychiatrie, von Damerow, &c. Band xi. Heft 2.
8. Vierteljahrschrift für die Prak. Heilk. Prag, 1854. Band 3.
9. Verhandlungen der Phys.-Med. Gesell. zu Würzburg. Band v. Heft 1.
10. Schmidt's Jahrbucher. 1854, Nos. 9, 10, 11.

FRENCH.

11. Archives Générales de Médecine. Sept., Oct., Nov.
12. Revue Méd. Chir. de Paris. Sept., Oct., Nov.
13. Bull. Gen. de Thérapeutique. Sept., Oct., Nov.
14. L'Union Médicale. Sept., Oct., Nov.

ITALIAN.

15. Bulletino delle Scienze Mediche. Bologna. Feb. April, May.

SPANISH.

16. Semanario Médico Español. Sept., Oct.

AMERICAN.

17. American Journal of the Medical Sciences. Oct.
18. Philadelphia Medical Examiner. August, Sept.
19. The American Journal of Insanity.
20. New Orleans Medical News and Hospital Gazette. Vol. I., No. 14.

35 years of age, weighed 53 kilogrammes, and was, at the time of observation, nursing a child; the fistula had existed for two or three years, and was caused probably by a perforating ulcer of the stomach. The *quantity* of the gastric juice, after abstraction of the saliva (65 grammes per hour) was computed at 584 grammes (=19 oz. nearly) for every hour, or 14,016 kilogrammes (=22¾ imperial pints nearly) for the day, which is about the fourth part of the weight of the whole body, a proportion much larger than that given by Bidder and Schmidt (6·4 kilogrammes for an individual weighing 64 kilogrammes);* 1 kilogramme of weight of a man would secrete, according to this calculation, 264 grammes (=8½ ounces nearly) of gastric juice in twenty-four hours. The smallest proportion was obtained in the morning (state of fasting), being, however, even then not less than between 40 and 400 grammes per hour. The fluid collected at that time was, in general, thin, serous, colourless, and clear; at other periods it was more viscid, containing sometimes bile, without any accompanying signs of functional derangement; not rarely sarcina was detected in it by aid of the microscope. As regards the *chemical constitution*, according to the examination performed by C. Schmidt, the fluid obtained at an early hour of the morning (state of fasting) was either neutral or slightly alkaline; after the ingestion of food it was always acid. *Hydrochloric acid* was not found in any of the analyses of the various portions of the gastric juice secreted at different times of the day, but *butyric* acid and the presence of *lactic* acid became highly probable. The following table shows the numerical proportion of the single constituents; for the sake of comparison we add the figures given by Bidder and Schmidt for the gastric juice of the *dog* and *sheep*.

	MAN. Gastric juice mixed with saliva.	DOG. Gastric juice without saliva.	DOG. Gastric juice mixed with saliva.	SHEEP. Gastric juice mixed with saliva.
Water	956·595	973·062	971·171	986·147
Total solids	43·405	26·938	28·829	13·853
Organic substances	36·603	17·127	17·336	4·055
Inorganic substances	6·802	9·811	11·493	9·798
Single substances:				
Hydrochloric acid	—	3·050	2·337	1·234
Chloride of potassium	—	1·125	1·073	1·518
Chloride of sodium	4·633	2·507	3·147	4·369
Chloride of calcium	—	0·624	1·661	0·114
Chloride of ammonia	—	0·468	0·537	0·473
Phosphate of lime	0·961	1·729	2·294	1·182
Phosphate of magnesia	0·260	0·226	0·323	0·577
Phosphate of iron	0·006	0·082	0·121	0·331
Potash, belonging to the organic substances	0·363	—	—	—

The organic substances consisted of coagulable albuminous substance (pepsin), sugar, butyric acid, uncoagulable proteinaceous substances, and lactic acid. The organic acids are not considered as originally present in the secretion of the stomach, but as products from the ingested aliments; they are changeable in quantity according to the quality of food. The hydrochloric acid, on the contrary, was considered as an essential component, although it is not exhibited by the analyses, probably on account of its being so easily bound by the alkalies of the saliva. In an analysis performed at a period, the results of which are communicated in the appendix, Schmidt actually found free hydrochloric acid, but only in the proportion of 0·2 in 1000 parts,—a proportion ten times smaller than in the dog. Concerning the question, whether the gastric juice prevents the saliva from transforming starch into sugar (as asserted by Bidder and Schmidt) the authors found that the action of the saliva is not annihilated; they confirm, however, the observation of Bidder and Schmidt, that no sugar is detected in the stomach of the dog even after the ingestion of boiled starch.

The digestion of *proteinaceous food* was examined by introducing weighed pieces

* Die Verdauungssaefte u. der Stoffwechsel. Mitau and Leipzig, 1852, p. 36.

of coagulated albumen, meat, &c., in thin linen bags through the fistula into the stomach, and observing both the loss of substance within a certain space of time, and the changes in the microscopic structure of the elements. Thus it was found that, for proteinaceous substances, the dissolving power of the gastric juice of man is far inferior to that of the dog; their solution is completed in the stomach of the dog within two to four hours, in that of man it requires nineteen to twenty hours. Raw meat is better digested by the stomach of man than boiled meat, and veal better than beef.

Concerning the *microscopic alterations*, they found after one hour and a half the primitive fasciculi easily separable from each other, without change in themselves, the sarcolemma destroyed; after two hours and three quarters, the primitive fasciculi commenced to exhibit transverse fissures; after three hours, only transversely striated lamellæ were visible; after three hours and a half, besides the quadratic lamellæ, many primitive fasciculi, fissured in longitudinal and transverse direction, frequently dentated at the extremities; after three hours and three quarters, the primitive fasciculi twice or three times divided in the longitudinal direction; after four hours and a half, scarcely any solid remains left in the stomach, except some few primitive fasciculi much fissured in the longitudinal and transverse direction, still, however, exhibiting the transverse striæ. After three and a half to four hours the stomach is, in general, empty; the proteinaceous substances become therefore subject to the influence of the enteric secretion, &c. As regards the digestion of *fat*, the membrane of the cells was dissolved in the stomach; but the fat itself did not undergo any alteration. *Milk*, after three quarters of an hour, formed a thick coagulum, enclosing a large quantity of milk globules and free fat; after two and a half hours, the casein was seen partly as amorphous substance, partly in transparent, membrane-like pieces, with some unchanged milk globuli; after three hours and three quarters scarcely any remains were left in the stomach.

These alterations, which required in the stomach of this woman three hours and three quarters to four hours and a half, were completed in that of a dog within two hours.

Finally, the authors allude to the great importance of the secretion of the gastric juice for the intermediate circulation of fluids and the intermediate change of matter, the principal object of which is to economise in the animal household, by prolonging the series of changes to which animal matter is submitted within the organism, and thus to render it for a longer time efficient in the discharge of vital processes. We find ourselves more compelled to adopt this view, as the quantitity of the secretion amounted in this instance to one-fourth of the weight of the whole body.

3. Kroeger gives in his dissertation the results of some experiments performed with Prof. Bidder. Through a fistula in the pancreatic duct they collected the pancreatic juice during certain spaces of time at various periods of the day and under various influences. Concerning the quantitative relations, 1 kilogramme of dog secretes, on the average, within twenty-four hours, about 89·3 grammes of pancreatic juice. According to this calculation, an adult man (weighing 64 kilogrammes) would secrete, in twenty-four hours, 5 715 kilogrammes, i. e., nearly the eleventh part of the weight of the whole body. The ingestion of food exercises great influence over the secretion, the latter becoming much increased in quantity almost immediately after meals, reaching its maximum within about half to three quarters of an hour after the meal, when it is about six or ten times larger than it had been just before the ingestion of food. Water has not the same effect: on the contrary, when taken simultaneously with solid food, it prevents the latter from causing so evident an increase. The concentration of the pancreatic juice appears frequently diminished in the same measure as the quantity is increased, but this phenomenon is not a constant one; and, at all events, the absolute quantity of solid substances is greater after meals than before.

As regards the physical and chemical character of the secretion the author does in general, agree with Bidder and Schmidt;* some differences, however, must be

* l. c. p 244.

noticed. The specific gravity is, according to Kroeger, 1·01065, while Bidder and Schmidt had assumed 1·0306, and Frerichs 1·0082.* Further differences become apparent by comparing the following figures relating to 1000 parts of fresh juice.

	Kroeger. (Analysis by C. Schmidt.)	Frerichs.	Bidder & Schmidt.
Water	981·52	986·40	900·76
Solid matter	18·48	13·60	99·24
Organic substance	12·68	3·50	90·38
Inorganic substance	5·80	10·12	8·86

Concerning the *physiological action* of the juice, its power of transforming starch into sugar is not doubted. According to the author's experiments, 1 gramme of the fresh juice transforms within half an hour, under the influence of a temperature of 35° C. 4·672 grammes of dry starch into sugar; as 1 gramme of fresh juice contains 0 014 grammes of pancreatic ferment, one gramme of this ferment would transform 333·7 grammes of dry starch. If we assume, with Frerichs, that an adult man requires daily about 490 grammes (=15 ounces nearly) of starch to compensate the daily loss of carbon, the quantity of pancreatic juice necessary for the transformation of this starch into sugar would be less than 105 grammes, while the quantity actually secreted amounts to more than 5000 grammes. Kroeger is therefore of the same opinion with Bidder and Schmidt—viz., that the pancreatic juice cannot have as its *principal* function the transformation of starch into sugar. As one of the functions, he considers the promotion of the constant interchange of fluids within the body, in the same manner as Bidder and Schmidt have made it so probable concerning the saliva, and with Grunewald and Schroeder concerning the gastric juice. It further appears to him that an intimate connexion exists between the secretion of the stomach and that of the pancreas—namely, that the hydrochloric acid secreted by the former is, after having performed its part, neutralized by the soda of the latter, thus again forming the chloride of sodium previously disunited by the process of secretion. In favour of this theory Kroeger observes that the hydrochloric acid secreted by 1 kilogramme of dog through the gastric juice in twenty-four hours amounts to 0·305 grammes, while that of soda contained in the pancreatic juice of twenty-four hours is calculated at 0·237 grammes, i. e., very nearly the equivalent (0·259) corresponding to 0·305 grammes of hydrochloric acid.

4. VALENTIN examined the gases of various parts of the intestinal canal of horses. The animals were killed by bleeding. Separate portions of the intestinal canal were isolated by double ligatures, and the gases collected immediately after death. We abstain from describing the chemical methods employed, but will construct a table showing the percentic composition of the gases of the two horses examined.

A. gives the figures belonging to a gelding, 20 years old, healthy, fed with oats and hay. The stomach contained a large quantity of food; the gases of the small intestines were collected from comparatively empty portions; the cæcum was in a great part filled with the remains of food; the gases of the rectum were from between the fæcal balls.

B. was an old mare, healthy, fed with oats. Stomach and cæcum were almost filled with solid masses. The rectum contained so small a portion of gas that no sufficient quantity for an analysis could be collected. The interval between the last meal and the death of the animal is not named.

* Wagner's Handwoerterbuch der Physiologie, iii. p. 844.

Names of the gases.	Stomach A	Stomach B	Upper portion of small intestines, A.	Middle portion of small intestines, B.	Lower portion of small intestines. A.	Cæcum. A	Cæcum. B	Middle portion of rectum, A.
Carbonic acid	44·35	55·64	18·83	41·78	19·41	77·70	71·59	47·94
Carbur. hydrogen	0·90	—	0·45	4·98	0·77	4·09	6·96	11·82
Sulphur. hydrogen	2·70	4·92	1·61	4·52	1·46	2·02	3·71	0·54
Hydrogen	0·66	13·29	—	0·02	0·08	4·67	0·20	13·82
Oxygen	7·16	0·77	5·76	—	4·97	—	—	—
Ammonia	—	—	—	—	—	1·29	1·22	1·49
Nitrogen	44·23	25·38	73·35	48·70	73·31	10·23	16·32	24·39
	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00

The coincidence of the large proportion of carbonic acid in the stomach and cæcum of both horses is remarkable, a circumstance probably connected with the functions which both organs have to fulfil in the digestive process.

The presence of sulphuretted hydrogen in the gases of all parts of the intestinal canal, makes Valentin conclude that the formation of this gas commences in the stomach. He considers the albuminous substances of the food to be the source of this formation, and draws the inference, that in the stomach not only a mere solution of some of the substances containing nitrogen and sulphur takes place, but that there is also a partial decomposition. It must, however, as yet remain uncertain how much of the sulphuretted hydrogen found in the stomach is due to a development of this gas in the stomach itself, and how much may have been introduced from lower parts by the diffusion of gases Ammonia, it will be seen, on the other side, is met with only in the large intestines. The proportion of hydrogen found by Valentin is smaller (except in the rectum) than that stated by former observers. He agrees, however, with them concerning the total absence of oxygen in the gases of the large intestines. The larger quantities of carburetted hydrogen and hydrogen in the rectum show that changes in the remains of food continue taking place up to the last portions of the digestive tube.

II. Blood, Respiration, Circulation.

1. *Contributions to the Physiology of the Blood.* By Prof. Vierordt. (Vierordt's Archiv, xiii. 3, pp 409. 1854.)
2. *On the Admixture of Ammonia in the Expired Air* (De l'Ammoniac dans la Respiration). By Profs. Viale and Latini. (l'Union Medicale, viii., No. 98, Août. 1854.)
3. *On the Admixture of Ammonia in the Expired Air.* By Dr. Reuling. Dissert. (Giessen. 1854.)
4. *Lithic Acid in the Tissue of the Lungs.* By Dr. Cloetta. (Virchow's Archiv, vii. 1; p. 168. 1854.)
5. *On the Action of the Heart in Various Gases.* By T. Castell. (A Successful Prize Essay at the University of Kœnigsberg. Muller's Archiv, 1854, iii. pp. 226, ss.)
6. *Observations on the Body of a Decapitated Criminal.* By Virchow, Kölliker, and others. (Verhandlungen der Würzburger Gesellschaft, vi. pp. 116. 1854.)
7. *Remarks on the Action of the Heart.* By Prof. Vierordt. (Appendix to the article above named—"Contributions," &c.)

1. Vierordt counted the blood globules in the blood of a marmot at various periods during the state of hybernation. Commencement of hybernation on November 22, 1853, when the weight of the animal was 845 grammes.

I. A few drops of blood were taken from the subcutaneous cellular tissue (the bloodvessels of which were remarkably full) on November 28th. The blood is of dark colour, coagulates quickly; number of blood globules in one cubic millimetre =5·748·000. (Weight of body not determined.)

II. On January 5th, 1854, weight of body=750 grammes. Blood does not so quickly coagulate; is of much lighter colour than it was on November 28th; number of globules in one cubic millimetre=5·100·000.

III. On February 4th, weight 613 grammes; the subcutaneous cellular tissue contains so little blood that the few drops for the examination must be taken from the jugular vein; the colour of the blood is very pale, the coagulation slow; number of globules=2·355·000. In consequence of these experiments, the little animal lost about six to seven grammes of blood, became extremely feeble, died on 18th February. Weight, eighteen hours after death, 537 grammes; number of globules 3·665·000. The gradual decrease of the weight of body, of the quantity and concentration of the blood, are very evident in these experiments. It is to be expected that further observations promised by Vierordt on the same subject will considerably add to our knowledge of the physiology of the blood and respiration.

Vierordt made also some observations concerning the proportion of the *coloured* to the *colourless globules* in the blood of the splenic vein of man. As the colourless globules possess a great tendency to join in heaps, this examination cannot be made so easily on man as on animals. Vierordt had the advantage of taking the blood of a decapitated criminal one hour and a half after death. The mean numbers of various countings give the figures 1 : 4·9 as the proportion of the colourless to the coloured corpuscles.

2. Viale and Latini, of Rome, are led, by their experiments, to the following inferences: 1. An emission of ammonia takes place in the act of expiration (0·763·026 grammes within twenty-four hours; 278 504·490 grammes within a year —which would make, in a town of 160,000 inhabitants, 44·560 kilogrammes). 2. The ammonia is then in the state of the sesquicarbonate. 3. The nitrogen mentioned by the chemists as a product of respiration, is only contained in the ammonia. 4. The ammonia developed in this way serves in a considerable degree to renew that which enters into the composition of the atmospheric air, and which, dissolved by rain-water, renders the earth fertile. 5. The contagious principle is, perhaps, nothing else but a salt of ammonia. 6. During the respiration an excretion of a certain quantity of albumen takes place which one might consider as ferment.

3. Dr. Reuling draws from his observations a conclusion very different to those just given, viz., that in the lungs of healthy persons ammonia is rather absorbed than developed. According to him the quantity of ammonia contained in the air of a healthy person, *expired* within twenty-four hours, amounts only to 18·72 milligrammes, which would be not more than 1·82 to 1,000,000 parts (by weight), while the same quantity of *atmospheric* air contains not less than 15·63 parts of ammonia. (See the Report on Medicine.)

4. Cloetta considers the presence of lithic acid in the lungs of herbivora as a normal phenomenon. He examined six ox-lungs, the aqueous extract of every one of which contained lithic acid. The following is the process adopted:—The minced lungs were exposed to the influence of distilled water during twenty-four hours. After this was as much as possible squeezed out, the albumen and colouring matter were precipitated by boiling, the fluid filtered, and baryta added until precipitation did not take place any longer. The fluid was filtered again, and placed on the water-bath, until it amounted to not more than 50 c.c. when the addition of acetic acid precipitated lithic acid crystals. The watery extract of the whole lung of an ox yielded 60 milligrammes of lithic acid.

5. Castell performed his experiments on the hearts of frogs, which he placed, immediately after their separation from the body, in glass cylinders filled with various gases. Temperature, 68°—77° F. The following table shows the average time during which the heart continued to move after having been exposed to the

influence of the respective gases. The O attached to a line means that the heart, when taken out of the gas and exposed to the atmospheric air, did not commence again to move; the — signifies that the atmospheric air excited traces of muscular action; the + that it effected regular contractions.

The heart of the frog continues to act	in the atmospheric air	during	180 min.	
,, ,,	under the air-pump	,,	30 min.	
,, ,,	under water	,,	20 min.	+
,, ,,	in oxygen	,,	720 min.	
,, ,,	in hydrogen (moist*)	,,	72 min.	+
,, ,,	in nitrogen	,,	68 min.	+
,, ,,	mixture of 2 nitro. and 1 carb. acid	,,	67 min.	+
,, ,,	hydrogen (dry*)	,,	47 min.	O
,, ,,	carbonic oxide	,,	44 min.	O
,, ,,	phosphoretted hydrogen	,,	27 min.	O
,, ,,	sulphuretted hydrogen	,,	12 min.	+
,, ,,	carbonic acid	,,	6 min.	+
,, ,,	protoxide of nitrogen	,,	5 min.	—
,, ,,	cyanogen	,,	4 min.	O
,, ,,	sulphuric acid	,,	3 min.	—
,, ,,	olefiant gas	,,	2 min.	O
,, ,,	chlorine	,,	2 min.	O
,, ,,	mixture of 4 nitrogen & 1 chlorine	,,	2 min.	O
,, ,,	iodine	,,	0 min.	O

The preceding table shows that the heart, in all the gases tried, with the exception of oxygen, ceases beating sooner than in the atmospheric air. The author concludes from this, that it is the presence of the oxygen in the atmospheric air by which the continuation of the action of the heart is rendered possible for so long a period after its separation from the body; while it is the want of oxygen to which the ceasing of the action is to be attributed. Not one of the gases which contain no free oxygen is able to keep up the action of the heart. The influence exercised by the single gases is apparently very different, some of them acting only by the want of free oxygen (the heart commences beating again, when soon after exposed to the atmospheric air); others destroying the irritability of the fibres of the heart, either by over-irritation, or by too strong an affinity to the elements of the heart (neither the atmospheric air nor any stimulants effect the re-appearance of action).

6. Kölliker and Virchow opened the pericardium of a decapitated criminal thirty-five minutes after death; the pericardium contained a small quantity of pale yellow transparent fluid, which formed soon after exposure to the air, a rather firm coagulum. Irritation (by electricity) of the sympathicus on the neck did not produce any action of the heart, but irritation of the substance of the heart itself excited local contractions. The aorta abdominalis contracted from sixteen to one millimetre; the vena meseraica, near the ilium, contracted about one-fourth of its lumen. A piece cut out of the *vena cava infer.* 39 millimetres long (sixty minutes after death), shortened by continued application of the stimulus to the length of 25 millimetres, forming many transverse wrinkles of the internal membrane. A piece from the vena portæ (after ninety-three minutes) contracted from 20 to 18 millimetres. The small pulmonary veins show decided contraction after fifty-five minutes.

7. Vierordt examined the action of the heart of a decapitated criminal by his instrument for the examination of the physical phenomena connected with pulsation (*pulsmaschine*). The heart was taken out immediately after death, and placed under the instrument with the right ventricle. The experiment commenced nine minutes after death.

* *Moist* hydrogen means hydrogen mixed with some vapour of water, through the process of development in distinction from *dry* hydrogen, which had been led through a tube containing chloride of calcium.

The average duration of the systole of the ventricles to that of the diastole was shown to be in the proportion of 10 to 65; the duration of the systole is therefore shorter than it appears to be to the eye.

Thirty-nine movements of the heart (each movement comprising one systole and one diastole) were performed in sixty-four seconds. The contractions following these were too indistinct to permit of being marked on the kymographion.

The duration of each movement is expressed in millimetres, by the following numbers:

I.	7·5	7·4	8·6	8·2	8·8	9·2	7·0	8·5	7·9	9·9
XI.	9·1	9·3	8·7	8·6	8·2	8·1	8·3	8·4	8·9	8·1
XXI.	17·0	9·0	5·2	12·1	9·7	8·8	18·2	9·9	9·9	10·0
XXXI.	10·2	10·0	24·6	10·1	9·2	18·6	11·8	10·7	10·0	

III. Lymphatic System and Ductless Glands.

1. *Observations on the Body of a Decapitated Criminal.* By Virchow, Kölliker, &c. (Wurzburg Gesellschaft, vi. 1854.)
   (See also, under II., "Contributions to the Physiology of the Blood." By Vierordt. On the proportion of the *coloured* to the *colourless globules* in the blood of the splenic vein.)

Virchow produced, by application of electricity to the ductus thoracicus, fifty-five minutes after death, intense contraction, continuous, not peristaltic. The contraction had, fifty minutes later, not altogether ceased. Several hours after death the irritability of the ductus thoracicus had not yet disappeared; therefore it remains longer in this than in any other organ. Also the small chyliferous vessels showed energetic contraction when stimulated by electricity, but here again the contraction was continuous, not peristaltic.

The *spleen* did not appear to contract, either in the longitudinal or transverse diameter, by the application of electricity; but a powerful stream made the impressions (corresponding to the insertion of the trabeculæ to the tunica), appear deeper than before. Another interesting phenomenon was a gradual change in shape. Soon after the abdominal cavity had been opened, the spleen was 95 millimetres long, and 68 or 69 millimetres broad; about half an hour later (seventy-two minutes after death), length, 102 millimetres; breadth, 65 millimetres; one hundred and eight minutes after death, the organ having been previously placed in tepid water, the length was 109 millimetres; breadth, 65.

IV. Organs of Secretion and Excretion. Skin.

1. *Contributions to the Knowledge of the Secretion of Urine in Healthy, Pregnant, and Diseased Persons; especially on the Quantitative Relations of the Phosphates.* By Dr. Mosler. (Dissert. Giessen, 1853.)
2. *Contributions to Urology, II.* By Dr. Beneke. (Archiv des Vereins für gemein: Arbeiten, No. 4, pp. 608. 1854.)
3. *Hippuric Acid in the Urine of Man.* By Dr. Duchek. (Prag. Viertelj. xi. 3. 1854.)
4. (Grunewald and Schroeder, "On the Gastric Juice." Kroeger, "On the Pancreatic Juice." See I.)
5. *On the Immediate Principles of the Excretions of Man and Animals in the Healthy Condition.* By William Marcet, M.D. (Proceedings of the Royal Society and "Medical Times and Gazette." Sept.)

1. Mosler made a series of observations partly on himself, partly on several other healthy persons. For the information concerning the individual circumstances of the subjects of examination, as also concerning the methods of analysis,

we must refer to the work itself. The average quantity of *phosphoric acid* secreted within twenty-four hours by a healthy man is calculated at 3·209 grammes; the acid contained in the alkaline phosphates to that in the earthy phosphates bears the proportion of 3 : 1. The quantity of colouring matter within twenty-four hours is about eight grammes. As regards the various periods of the day the secretion of phosphoric acid was greatest in the evening, when the author was, in general, mentally engaged; after this follow in a descending series, noon, night, and morning. By intense mental occupation the quantity of phosphates excreted became regularly increased one-half, the increase relating more to the earthy than the alkaline phosphates; the quantity of colouring matter became likewise greater. By an abundance of proteinaceous food, the excretion of phosphates became in a similar proportion larger. The effect of both influences combined on the excretion of phosphoric acid, urea, and chlorine, is striking in the following table under A, while the figures found in normal circumstances are placed sub B.

	Urea.	Chlorine.	Phosphoric acid.
A	43·050 grammes.	20·720 grammes.	5·104 grammes.
B	29·120 „	7·010 „	4·479 „

By fasting, the excretion of phosphoric acid was more diminished than that of urea, the quantity of chlorine in the urine remaining unaltered. The examination of a non-pregnant female, extending over five days, gave much lower figures for the phosphates, than of men under similar circumstances. Concerning the urine of pregnant females, the author found, as the mean figures of several examinations for women of 128 pounds' weight, after the sixth month of pregnancy: quantity of twenty-four hours = 1·488 C.C. (62 C.C. per hour); specific gravity = 1·011; reaction more frequently neutral or alkaline than acid; colouring matter 14·592 grammes (0·608 per hour); urea = 26·193 grammes (1·090 per hour); chlorine = 7·930 grammes (0·330 per hour); phosphoric acid = 2·422 grammes (0·100 per hour); sulphuric acid = 1·250 grammes (0·052 per hour).

2. Beneke dwells principally on circumstances connected with the quantity of urine, its specific gravity, and the amount of phosphoric and sulphuric acid contained in it. We must refer, as regards the manner of observation, &c., to the paper itself, restricting ourselves to the principal points discussed in it.

I. Concerning the *quantity* passed in twenty-four hours, he says that it is in some instances larger than the quantity of fluids taken as *drink*, in others smaller; in the former case the excess of the fluid in the urine is, according to all the observations made on man and animals (Bischoff,* C. Schmidt,† &c.), derived either from the fluids contained in the solid food, or from the organs of the body itself; this latter circumstance being almost always connected either with diminished activity of the skin, or with an irritated state of the nervous system. The *quality* is of great importance on the quantity of urine excreted within a certain space of time; abundance of meat produces, as well in man as in animals, considerable increase, which Dr. Beneke is inclined to attribute to its stimulating action on the nervous system. He alludes to another circumstance of importance connected with the quality, the *endosmotic equivalent.* We know, for instance, that each equivalent of sugar entering into the blood assumes for itself seven equivalents of water; that albuminous substances do not require quite as much; that chloride of sodium has a much higher equivalent: by the introduction and excretion of bodies with a high equivalent the body may be deprived of a large quantity of fluids.

II. Concerning the *specific gravity*, and the excretions of solid matters with the urine, the author reminds us that the quantity of water is not necessarily proportionate to the quantity of solids contained in the urine. As regards the various periods of

* Bidder and Schmidt, Verdauung u. Stoffwechsel, 1852.
† Bischoff, der Harnstoff als Maass d. Stoffwechsel, 1853.

the day he agrees with Vogel, that the quantity of solid substances excreted per hour is greatest during the afternoon and the evening (after food), smaller during the night and the morning. Beneke draws our attention to the importance of observations on the influence of temperature on the excretion of solid substances, respecting metamorphosis of matter. Summing up our knowledge of the action of the various points in question, he says, "Increase of the ingestion of water increases the egestion of water; increased ingestion of solids produces an increased egestion of solids (afternoon urine). As yet it remains undecided whether an increased ingestion of water alone causes an increased egestion of solids, but it appears to be certain that an increased ingestion of solids does not necessarily effect an increased egestion of water, unless the *quality* of the food (meat, &c.) be acting as an excitant on the nervous system (Bischoff); influences exercising a depressing action on the nervous system effect a decrease of the excretion as well of solids as of water; influences of an exciting character have the opposite effect; as regards the *solids*, the change does *always* appear in the urine; as regards the *water*, it does not *necessarily* so, as there are various ways for the elimination of water.

III. *Phosphoric acid.* The average quantity of twenty-four hours, in one series of observations, was 2·946 grammes, in another 2·183 grammes, which is rather less than Winter* and Mosler† had found. The proportion of the acid combined with alkalies to that combined with earths, was in the average = 2 118 : 0·663 (determined only in the first series). Concerning the distribution of the excretion over the single periods of the day, the highest figures belong to the afternoon and evening, the lowest to the morning (dinner taken early in the afternoon).

IV. The quantity of *sulphuric acid* of twenty-four hours ranged between 1·8404 and 2·819 grammes, which is quite in accordance with Gruner's‡ observations. Here again we find the highest figures for each hour in the later part of the afternoon and evening (0 0866 to 0·1269 grammes), the lowest in the morning (0 0321 grammes).

3. Duchek found hippuric acid in his urine whenever he had taken green prunes (the ripe fruit of *prunus domestica*—var. chlorocarpa). The analysis of these prunes exhibited a considerable quantity of benzoic acid. The hippuric acid began to appear about seven to eight hours after he had eaten the prunes, and ceased to appear from three to five hours later. Supposing that hippuric acid is a conjugate acid, consisting of benzoic acid and glycocoll, the author made some experiments concerning the quantity of glycocoll which the healthy human organism can yield under the influence of a merely animal diet. He examined, therefore, the urine after having taken weighed quantities of benzoic acid. The ingestion of 1 gramme of benzoic acid was followed by the excretion of 0·714 grammes of hippuric acid; 2 grammes of benzoic acid by 1·857 grammes of hippuric acid and 0·421 grammes of benzoic acid; 4 grammes of benzoic acid by 1·714 grammes of hippuric acid, and 2·500 of benzoic acid. Duchek concludes from this, that within a fixed space of time the organism can dispose only of a certain amount of glycocoll, which must be either detracted from the fabrication of bile or exist in superabundance. If the former supposition were correct, the fabrication of bile would be impaired by taking benzoic acid; while, according to the other supposition, it would act as an indifferent body; the author is inclined to adopt the latter view, as the ingestion of larger quantities of benzoic acid appears to be without any influence on the state of health.

4. A very interesting series of observations has been made by Dr. Marcet on excrement. He has discovered a new immediate principle in the fæces of man which he terms excretine. This principle crystallizes out of alcohol or ether in

* Bestimmung d. Phosphate u. d. freien Sæure im Urin. Giessen, 1852.
† Analyzed just before.
‡ Die Ausschirdung d. Schwefelsæure durch d. Harn. Giessen, 1852.

beautiful four-sided prisms; its exact composition has not been made out, but it contains both nitrogen and sulphur though in small proportions. Besides excretine, Dr. Marcet finds margaric acid, colouring matter similar to that of blood and urine, and two other organic substances, one of which is probably a mixture of bodies and the other is a fatty acid which it is proposed to call excretoleic acid in human fæces. He has found no butyric or lactic acid.

In animals fed on meat (dog, leopard, tiger), a substance analogous to, but not identical with, excretine was found. Butyric acid was also discovered.

The fæces of herbivorous animals (horse, sheep, dog, when fed on bread) contained no excretine, no butyric acid, and no cholesterine.

The excrements of the crocodile contained cholesterine but no uric acid; those of the boa, uric acid, but no cholesterine.

The report before us does not mention what amount of excretine is contained in human fæces, and whether it constitutes the bulk of them. We shall, however, return to the paper when the 'Transactions of the Society' are published.

V. Nervous System.

1. *De Actione Belladonnæ in Iridem.* By G. C. P. De Ruiter. (Dissert. Trajecti ad Rhenam. 1853.)
2. *On the Action of the Nervous Splanchnicus Major.* By Dr. Haffter. (Dissert. Zurich. 1853.)
3. *Observations on a Decapitated Criminal.* By Drs. Müller, Leydig, &c. (Verhandlungen der Würzburg Gesellschaft, vi. pp. 116, ss. 1854.)

1. Ruiter considers the action of the iris as twofold: 1, *reflex*, by the rays of light acting on the retina, and only through this on the iris; not adopting the view of Harless* and Budge,† that the light acts directly on the iris; 2, *voluntary*, for the accurate examination of near objects. Ruiter calls this voluntary, because we are conscious of the effect of the contraction, although we are not conscious of the contraction itself. Ruiter confirms the experiments of Budge, Waller, and Wagner, that the pupil becomes immediately smaller by section of the nervus sympathicus (in frogs and rabbits), and of the nervus vagus (in dogs) of the same side. In rabbits and frogs the proportion of the dimension of the pupil of the side operated upon to that of the other eye, was as 2 : 3; in dogs as 1 : 3. Several days after the section the difference was still visible, but not so great. Ruiter considers, therefore, the sympathicus nerve as the nervous *dilatator* of the pupil. He agrees with Budge, in opposition to Volkmann and E. Weber, concerning the supposition that the nervus oculomotorius regulates the action of the sphincter pupillæ; on the ground as well of the physiological experiment as also of the pathological fact, that paralysis of the nervus oculomotorius is always accompanied by dilatation of the pupil. To the fifth pair he attributes the sensibility of the iris; but, besides the sensitive functions, he is inclined to ascribe to it some motor influence on the iris, as section of this nerve on the base of the brain, and also irritation of the nervus ophthalmicus Willisii (Budge, Waller,) are followed by some contraction of the pupil, which, however, is less considerable and of shorter duration than that produced by section of the sympathicus. As regards the *modus operandi* of the atropia, we restrain ourselves to the two following inferences of the author: 1. that it diminishes the energy of the musculus sphincter pupillæ (scil., filaments of the nervus oculomotor); 2. that it does not diminish, but probably increases the action of the musculus dilatator pupillæ (scil., filaments of the nervus sympathicus)..

2. H. Müller applied a powerful stream of electricity to the nervus sympathicus of the left side, near the ganglion cervicale supremum (about forty minutes

* Die Muskelirritabilitael. München, 1850. † Comptes Rendus, tom. xxxv. p. 561.

after the decapitation, the head being separated at the sixth cervical vertebra); he observed immediately a considerable contraction, less, however, than can be produced by belladonna; the dilatation lasted as long as the irritation was continued; as soon as the latter ceased, the pupil returned to its normal size, but less rapidly than the dilatation had taken place. There was no difference in the effect whether the stream was applied to the upper or lower extremity of the ganglion. The pupil of the other side remained unchanged.

Irritation of the nervus vagus did not influence the pupil. Irritation of the nervus oculomotorius, ninety minutes after death, was likewise without effect on the pupil.

LEYDIG could not detect ciliar action in the lateral ventricles of the brain (as Valentin and Purkyni had done), but he found it very distinctly in the fourth ventricle; the cells were of a round rather flat shape, with long cilia. By the application of liquor potassæ, the action of the cilia was once more reproduced after it had already ceased.

3. HAFFTER, in connexion with Professor Ludwig, divided, in five cats, the nervus splanchnicus externally to the peritoneum. Concerning the *sensitive* function, it appears to contain a large amount of sensitive fibres, as the signs of pain produced by the section of the nerve are as great as those by the section of an equally thick branch of the trigeminus The perception of *hunger* was not destroyed after the operation; as the section of the *vagus* does likewise not annihilate it (Bidder and Schmidt), Haffter concludes that it depends on the combined action of the nervus vagus and nervus splanchnicus. As regards the *motor* function, the author is led to conclude from his experiments that the nervus splanchnicus neither *excites* the motion of the intestines, nor possesses the power of *arresting* it as the vagus that of the heart. The *secreting* function of the intestines became only slightly altered; the secretion of the stomach, however, and of the upper part of the small intestines, seemed to be rather increased. The nutrition, as well as the secretion of the kidneys unimpaired. The colour of the liver unusually dark.

VI. ORGANS OF SENSES.

1. *Contributions to the Physiology of the Sense of Sight.* By Dr. L. FICK. (Muller's Archiv, 1854, iii.)
2. *On the Accommodation of the Eye.* By Dr. CZERMAK. (Prag. Vierteljahrs. xi, 3. pp. 109.)
3. *Observations on a Decapitated Criminal.* (Verhandl. der Wurzburg Gesellschaft, vi. pp. 116. 1854. Compare also No. 1 and 3, sub. v.)

1. L. FICK considers the question—"Why do we see all objects as they are, and all parts of objects in their correct relation to each other, i. e., right, where is right? left, where is left? above, which is above, &c.? while it is an undeniable fact that an inverted image is thrown on the retina?" None of the explanations as yet given appear satisfactory to the author. Before giving his own, he reminds us that the soul forms the perception of objects by means of the impressions conveyed to the brain from the periphery through the nerves, the central ends of which must be arranged in such a manner as to produce impressions accurately corresponding to the external objects. His explanation is then that the central ends of the optic nerve are implanted into the brain in a manner perfectly opposite to the arrangement of the peripheric ends in the retina.

2. CZERMAK agrees with Cramer and Helmholtz that the interior changes in the eye during the process of accommodation for near objects, consist in the increase of convexity of the anterior surface of the lens, while the convexity becomes diminished during the accommodation for distant objects. That the lens does not at all change its place, is proved by the fact that during the accommoda-

tion the first and third of the three little images in the eye, i. e., that caused by the corner and that caused by the posterior surface of the lens, do not alter either in position or in size, while the image, during the accommodation for near objects, advances and becomes smaller. Concerning the means by which this change in the shape of the lens is effected, Czermak again agrees with Cramer in finding it in the iris and tensor choroideæ. Irritation of the iris by electro-magnetism produces the alteration in question; the same irritation effects no change as soon as the iris is cut from the pupillar margin to the periphery. Czermak is, however, of opinion that the iris is assisted in action on the shape of the lens by the *uvea*. In a former volume of this journal* we have mentioned Fick's view, that the *uvea* is the apparatus which effects, besides the regulation of the quantity of light, the accommodation for near and remote objects, by means of a change of the position and form of the lens, placing varying quantities of blood now before then behind the lens. Czermak considers Fick's view by itself as insufficient to explain the process; a varying quantity of blood, he reasons, would be necessary. Cramer experimented, however, on eyes just cut out, removed therefore from the circulation and the possibility of a change in the quantity of blood. Czermak adopts only that part of Fick's view by which is assumed that, through the filling and emptying of the processes ciliares the shape of the lens becomes changed in the way described.

3. Leydig, H. Muller, and Gegenbaur found ciliar action on all parts of the nasal cavity, also on the regio olfactoria of the ethmoidal bone. A distinct direction in the movement to either side could not be detected. Kölliker discovered the presence of *cavernous* tissue in the inferior concha with organic muscular fibres and manifest contraction under the influence of electricity.

Kölliker did not detect any ciliary epithelium on the membrane tympana, but he found it over the promontorium and most other parts of the tympanum.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.†

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1. *On the Occurrence of Leucin and Tyrosin in the Human Liver.* By Professor F. T. Frerichs and G. Staedeler. (Muller's Archiv, Heft 4. 1854.)
The same. By Professor Frerichs. (Correspondenz Blatt des Vereins fur. weiss. Heilk. und Wien Méd. Wochenschrift, 1854, No. 30.)

In 1851, Professor Frerichs observed in a liver affected with acute yellow atrophy, a number of needle-formed crystals mixed with destroyed liver cells; they were also present in the blood in the hepatic vein. They were not sufficiently numerous to permit an analysis to be made, and in other livers they were looked for in vain. In 1853, in the liver of a woman who died comatose with closure of the ductus communis choledochus, similar crystals were found in great numbers. The liver was now treated in a manner described fully by the writer, but which is too long to insert here, and both leucin and tyrosin were unequivocally proved to exist.

The authors then discuss, at some length, the alliances of tyrosin to the salicylic group, and give reasons for supposing tyrosin to contain the elements of glycin and saligenin with two equivalents of water, but from want of material they leave the subject only partly explored; but they remark, that in the urine of

* No. 27, p. 257.

† We have been obliged to defer several analyses of long foreign papers till our next number, and this has made the Report shorter than usual. As the number of papers analyzed is so few, we have not attempted any arrangement of them.

beasts and man, compounds derived from this group of bodies can be found. Thus, they allude to the occurrence of carbolic acid in the urine of herbivoræ, and to the observation of Sicherer (Hassall?), that indigo can be obtained from the urine of men. They think that these bodies may be derived from saligenen formed in the decomposition of tyrosin in the liver, the glycin then set free being used in the formation of glycocolic acid.

With reference to leucin they remark, that its oxidation furnishes valerianic and butyric acid; but, as they observe, these substances are also derived from the oxidation of fat and other substances.

The authors then state, that they believe leucin and tyrosin to be formed very early in the body, and to be destroyed in the liver. In healthy livers they have never found them; whereas, when the action of the liver is destroyed, they are found in abundance. With respect to the supposition that the leucin and tyrosin might be found before or shortly after death, by decomposition of protein substances, they observe that the livers they examined had no trace of decomposition, and that if so formed, the quantity of tyrosin would have been small, instead of being so great as in the last case referred to.

Both bodies have been found in the liver of variola and typhus patients (typhus abdominalis). In these patients, also, leucin and valerianic acid were found in the urine.

As in variola and typhus there are similar symptoms of nervous commotion as in the cholæmia of the acute yellow atrophy, the authors conjectured that the cause might in all cases be the same—viz., the presence of leucin and tyrosin in the blood; but on injecting both bodies, and also valerianate of ammonia, into the blood, no poisonous effect was produced. The leucin was easily found again in the blood, but not so the tyrosin—it was perhaps destroyed in the liver.

In a note it is observed, that Dr. Valentiner some time ago discovered leucin in the urine of an epileptic patient.

The views of Professor Frerichs, as given in the second work quoted above, although in part a recapitulation of what has now been said, are of sufficient importance to lead us to translate them:

"1. In the acute atrophy and softening of the liver, large quantities of leucin and tyrosin form in this organ, and pass from thence in part into the blood.

"2. The cholæmic intoxication is not the consequence of the accumulation of bile in the blood, but occurs in connexion with the formation of this product of albuminous decomposition in the liver.

"We have observed this intoxication only when many of the crystals of leucin have been separated in the parenchyma of the liver, and in the hepatic veins. The injection of great quantities of filtered bile never produced disturbance of sensorial activity. The bile entirely disappeared; once only when a concentrated solution of pure bile-acids and soda was injected in very great quantity, could remains of unchanged bile be found in the urine. I have vainly sought to know the various intermediate products which are formed before the bile is perfectly destroyed, nor have I discovered where the Taurin, a body which changes with so much difficulty, remains. At present this inquiry is being pursued in concert with Dr. Baumert.

"3. In typhus and exanthematous diseases, small-pox, &c., leucin and tyrosin form in the liver.

"4. The liver in this way contributes essentially to the origin of the blood-alteration, through which these pathological processes are characterized.

"5. Leucin and tyrosin are found in the blood of typhus and variola patients; they pass partly unchanged, partly decomposed, in the secretions and excretions, and are in this way separated.

"6. The formerly hypothetically received abnormal composition of the blood in these diseases has received in this way actual proof. The nitrogenous organic matters suffer here, in fact, in part, another metamorphose than the customary one, which ends in the production of urea."

2. *On the Employment of Hydrochloric Acid as a Means of Discovering the Contamination of the Blood by Carbonate of Ammonia.* By Dr. METTENHEIMER. (Archiv des Vereins für wiss. Heilkunde, Band i., Heft 4.)

On the Ammonia Constituent of the Expired Air, with reference to Uræmia. By W. REULING. (Inaug. Dissert. Giessen, 1854.)

METTENHEIMER observes that the breath of many quite healthy persons produces as much cloudiness about a rod dipped in hydrochloric acid as the breath of an uræmic patient, and that from this test nothing can be decided. This is the same conclusion as that to which Guterback and Schottin* had already come.

This question has been investigated most perfectly by Reuling, who, by means of delicate reagents and a particular apparatus, has absolutely proved that the breath of all men, under the most various conditions, contains ammonia. The quantity, however, thus appearing in the breath is considerably less than that normally contained in the atmosphere, so that there is no proof that the ammonia is given off by the pulmonary mucous membrane, and that it is not merely a portion of that which had been the moment before inhaled. It might, indeed, be rather supposed that it is absorbed from the atmosphere; but Reuling, on careful investigation, does not adopt this idea, as he found no ammonia in the healthy blood when it was first removed from the body, although ammonia soon appears in it.

With respect to a test for ammonia in clinical investigations, Reuling objects, on many grounds, to the rude criterion of a rod dipped in hydrochloric acid. The reagent he employs is prepared as follows: A tincture is made of four to eight drachms of logwood, four ounces of alcohol, and twelve ounces of water, and is mixed with sixteen to twenty grains of alum (free from iron), and with three to four drachms of fused chloride of calcium, dissolved in water, and acidulated with diluted hydrochloric acid to such an extent, as to give a feeble redness to litmus-paper. A few drops of diluted hydrochloric acid are added to the solution, and then paper which has been freed from iron and lime by washing with hydrochloric acid and distilled water, is wetted with it. The paper should take a cherry-red colour; if it is purple-red, enough hydrochloric acid has not been added to the solution; if, on the other hand, too much acid is added, the paper takes a citron-yellow colour. When the proper colour has been given, the paper is quickly dried near a warm oven, and is then placed in bottles, which are to be hermetically sealed, and kept from the light. Ammonia in the air gives the paper a blue tint, and so delicate is the test, that a fluid which holds only $\frac{1}{100000}$ of free ammonia produces the reaction, if the paper be suspended over it. By its means, Reuling has found ammonia in distilled water, vinegar, &c.

The breath has been examined in a great number of diseases, but we shall give merely the principal results. The amount of ammonia in it was found to be increased when the teeth were carious, when there was angina and abscess in the tonsils: in a case of typhus (abdominalis) with albuminous urine (while in 14 other cases there was no increase); in 2 cases of pyæmia in dogs (the pyæmia being artificially produced); in two dogs in whom the kidneys were excised; in 3 cases of uræmia in men (while in a fourth well-marked case there was no increase); in a case of uræmia, produced by blennorrhœa of the bladder, with healthy kidneys. The author thinks, also, that, most probably, it is increased in cholera and in scarlatina.

The expired ammonia was *not* increased in scorbutus, icterus, syphilis, variola, measles, intermittents, carcinoma, dropsy, acute and chronic bronchitis, œdema pulmonum, pneumonia and pleurisy, empyema, tuberculosis, acute gastro-intestinal catarrh, chronic ulcer of stomach, dysentery, peritonitis, epilepsy, neoplasma cerebri, and cerebral and spinal commotion.

With respect especially to uræmia, the author's own words are: "The increase of the ammonia-constituents of the expired air occurs most frequently in uræmia, but is no pathognomonic sign of this disease. The appearance of ammonia in the

* See No. 23, p. 268.

blood is, indeed, the most frequent, but is not the only cause of uræmia. It would also appear that, in sensitive individuals, uræmia can arise when, from impeded renal excretion, extractive matters accumulate in the blood. .... Uræmia must be cut out from the list of independent diseases, because it exhibits no essential peculiarities in the nervous phenomena which are observed so frequently in inflammatory fevers, acute exanthemata, typhus, and many poisonings."—(p. 49, 50.)

3. *On Paracentesis of the Pericardium.* By Drs. TROUSSEAU and LASEGNE. (Archives Générales, Nov.)

M. TROUSSEAU records the following interesting case: A man, æt 16, was admitted on the fifth day of an acute disease, with severe frontal headache, lassitude, and præcordial pain. There was intense dyspnœa, a little cough, a very quick pulse, prominence in the cardiac region, increased percussion dulness at the same point, extending to the second rib, and to the right of the sternum; the heart's sounds were feeble and distant. No mention is made of rheumatism, and it is to be presumed that it was absent. For a month the effusion continued the same; only once, for two days, did it seem to diminish, and then there was a little friction at the base. Afterwards, the dyspnœa increased and the fluid augmented as the dulness now reached to the clavicle. There was also pleuritic effusion. It was determined to puncture the pericardium: This was done by an incision in the fifth intercostal space, three centimetres (= $1\frac{1}{4}$ inch) from the sternum; thirteen ounces of fluid slowly escaped, the cardiac dulness decreased in amount, respiration could be heard in the lung as low as the fourth rib. A day or two afterwards, the pleuritic effusion was found to have increased; the heart was displaced to the right. Thoracentesis was practised in the sixth space in the axillary line, and although at first the canula was blocked-up by false membrane, eventually sixteen ounces of fluid were removed. Neither pleural nor pericardiac effusion reappeared, but, soon after, signs of tuberculosis of the left lung came on.

The authors refer to the other (7) cases of paracentesis pericardii on record.

4. *On Calculous Phthisis.* By Dr. FORGET. (Revue Méd. Chir. de Paris, Oct.)

BAYLE gave the name of phthisis calculosa or cretacea to those cases of consumption in which there were cretaceous, ossiform concretions in the lungs. Later writers have looked upon these as the remains of tubercle, and M. Forget does not dissent from this view. He asserts, however, that there is in addition a form of phthisis in which such concretions are primitive, i. e., do not follow tuberculous exudation. He relates two cases with the physical and general symptoms of phthisis, in which an osseoid mass being coughed up, one patient got perfectly well, and the state of the other was materially benefited. A third case is related, in which a girl having died of variola, a cretaceous mass was found in the apex of one lung, without trace of tubercle round it. On these three (and, we must say, imperfectly recorded) cases, the author bases the following conclusions:

1. The pulmonary calculi may be primitive, *sui generis*, that is to say, independent of tubercles, or of inhalation of dust, &c.
2. The calculi may be solitary.
3. They may remain latent in the lungs for a long time.
4. They can cause the same symptoms as tuberculous phthisis.
5. Phthisis calculosa can be entirely cured by the expulsion of the calculi.
6. Phthisis calculosa is a special malady differing from tuberculous phthisis by its anatomical characters, and by its terminations.

5. *On the Occurrence of Allantoin in Urine in cases of Impeded Respiration.* By Profs. FRERICHS and STAEDELER. (Müller Archiv, 1854. Heft 4.)

The writers desired to test the assertion of Alvaro Reynoso, that sugar appears in the urine when the respiration is much interfered with, but found, as others

have found,* that, although sometimes there was a doubtful reaction with the copper test, it was not sufficient for complete proof. On investigating the cause of the doubtful reaction, they discovered that it was owing (sometimes, at any rate,) to the presence of allantoin, as this substance was found in the urine of two dogs, the action of whose lungs was artificially impeded by injecting oil into the bronchi, or by the inhalation of chlorine. In the case of a man there was a doubtful trace. Observations were made in various diseases with impeded respiration, such as empyema, pneumonia, and aneurism of the aorta, but no allantoin was discovered.

With reference to the fallacies of the copper test, it is remarked that, unlike allantoin, kreatin has no power of causing the precipitation of the oxide of copper.

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6. *The Determination of Iron in the Urine.* By Dr. F. W. Böcker. (Prag. Vierteljahrsch. 1854—3.)

The method proposed by Böcker is by the use of a solution of permanganate of potash of a certain strength. A measured portion of urine (say 100 c. c.) is evaporated to dryness, and thoroughly incerated. The residue is dissolved in pure hydrochloric acid, and, at the same time, half a drachm of zinc is added, and as much hydrochloric acid as will dissolve the zinc. A little carbon always remains undissolved, and is well washed with distilled water, the washings being added to the acid solution, until the solution amounts to 4 or 5 ounces. Then it is filtered, and the permanganate is added drop by drop, till a fine rose red colour is produced. The quantity of permanganate used is then read off, and from this the quantity of iron is calculated. The strength of the solution used by Böcker is so prepared that 45 c. c. correspond to 0·100 grammes of iron.

The only possible error is, that the purest zinc always contains iron. To rectify this, it is necessary to make a second experiment with zinc and acid alone, and to see how much of the solution of the permanganate is used before the fine rose colour appears.

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7. *The Elimination of Urea by the Skin.* By C. H. Fiedler. (Inaug. Dissert. in Schmidt's Jahrb. 1854. 10.)

Schottin has already shown, that in some cases of cholera, and morbus Brightii, urea is found in the sweat. Fiedler relates two cases of Bright's disease in which this occurred. In one case there was suppression of urine, in the other there was sufficient secretion; but the analysis of this urine is not given.

QUARTERLY REPORT ON SURGERY.

By Holmes Coote, Esq., F.R.C.S.,
Assistant-Surgeon to St. Bartholomew's Hospital.

Upon the Cicatrization of recent Wounds produced by Caustic. By Dr. Girouard.—Dr. Girouard, of Chartres, read before the Association Médicale d'Eure et Loire, Feb. 27, 1854, an interesting essay, purporting to show in what manner recent wounds produced by caustic, healed, and how unseemly or inconvenient cicatrices might be prevented. Old wounds do not pursue quite the same course as recent ones.

After the application of caustic (Vienna paste, &c.) to the integument of a thin subject, the edges of the wound, when burnt perpendicularly from the surface, become thinner from the very day of the separation of the eschar; the day follow-

* See this journal for January, 1853.

ing they form an inclined plane; and a ring of granulations, one line wide and of deeper red colour than elsewhere, forms a zone on the free margin of the wound. On the fourth day the zone becomes narrower and whitens, and forms a cicatrisial membrane; then a new red zone is developed within the preceding, and thus the wound becomes smaller from day to day. In fat subjects, in whom the areolar tissue is charged with adipose matter, the edges of a similar wound undergo but little or no "thinning;" they round themselves; the adipose membrane swells, becomes covered by granulations; the zone then forms, and, rising to the surface of the wound, pursues the same course as above described. When the edges of the wound are so burnt or cut as to be oblique, the cicatrisial zone pervades, it is true, all the phases of the work of healing, but the skin, loosely connected to the subjacent parts, is dragged towards the centre of the wound. The cicatrix becomes in time condensed and contracted.

The daily diminution of the size of the wound always corresponds with the width of the zone—namely, two to three millimetres, or one line to a line and a half. Centres of cicatrization occur only in old wounds.

By constantly destroying with caustic the cicatrisial zone, the process of healing is permanently arrested; and unseemly contractions may be avoided—first, by limiting the cicatrising process to situations where the contracting force of the granulation can exercise no inconvenience; secondly, in so arranging the shape of a wound that the extremities of the cicatrisial radii terminate in very loose and extensile tissues; thirdly, in so shaping the edges of the wound that the contracting forces neutralize one another. The Mayor of Ponthevrard had on the lip a cancerous growth, which was removed by caustic. The eschar separated on the eighth day. By means of escharotics, the cicatrisial zone was continually destroyed along the line of wound corresponding with the external integument: the mucous membrane was, then, the part furnishing the cicatrisial granulations; and as contraction was confined to that side of the wound, the mucous membrane was drawn outwards, so as to form a very perfect lip.

Upon Epispadias. By M. Nelaton.—Nelaton, in the 'Gazette Hebdomadaire,' remarks that epispadias is a fissure of the corpora cavernosa, just as hypospadias is a fissure of the corpus spongiosum, or nymphæ masculinæ. But as the corpora cavernosa and the pubic symphysis form parts of the same system, the deficiency in one implies a corresponding deficiency in the other; and so it can be readily understood how, with a little further arrest of development, epispadias may pass into extrophy of the bladder. A Swede, æt. 20, who came from Stockholm with the express purpose of putting himself under M. Nelaton's care, presented the following appearances: "The separation of the two corpora cavernosa left upon the upper surface of the penis a shallow urethral gutter, covered by a highly sensitive mucous membrane, and terminating posteriorly in a rounded funnel, on a level with the lower border of the sub-pubic ligament. The pubic bones were one inch apart. Deeply seated at the bottom of the funnel was the urethro-vesical orifice. The patient wore an apparatus to retain the urine during the day; at night time, in spite of the contrivance of a funnel passing through the mattress, he lay constantly in a sort of urinal: his sufferings may be readily imagined." M. Nelaton operated in the following manner. He formed, at the expense of the skin of the abdomen immediately above the urethral infundibular, a quadrilateral flap, as wide as the penis, and rather longer than the organ. He dissected it off, so as to leave it attached at its inferior border by a broad pedicle, corresponding with the inter-pubic ligament. Thus he formed a large cutaneous flap, with which he proposed to close the urethral gutter. The next step consisted in attaching the edges of the flap to the penis. M. Nelaton made, on either side of the penis, on its upper surface, at the junction of the skin with the urethral mucous membrane, an incision extending to the base of the glans. Two short transverse incisions, at the extremities of the preceding, enabled him to dissect back flaps a third of an inch broad, destined to overlap and hold in place the large

piece of integument to be brought down from the abdomen; the raw surfaces of the two flaps formed on the penis would then correspond with the raw surface of the abdominal flap. This having been effected, and the parts united by harelip pins and sutures, two longitudinal incisions were made through the integument of the under side of the penis, that the skin of the organ might be loose, and not drag on the raw edges of the wound above. At the end of three days the sutures were removed; the edges gaped a little, but were soon filled up by granulations. At the end of a month the urethra was quite closed, but was wide enough to admit the introduction of the finger. To rectify this condition, and to promote contraction of the urethra, M. Nelaton employed repeated cauterizations on the inner surface of the new skin, care being taken to avoid burning the mucous membrane. At the end of the eighth or tenth day, when the eschars were about to separate, M. Nelaton made longitudinal incisions to favour contraction, and by these means a state of parts was produced, which may be described as follows: The patient still suffers from polyuria, but he can hold his water when he is in bed; secondly, when he is seated; thirdly, when he is upright and making no effort. When walking, he wears the ordinary caoutchouc apparatus. A similar operation, with beneficial results, was performed by the same surgeon, in 1852, upon another young man, in a yet more deplorable condition.

Operations undertaken for the relief of congenital deformities of the urinary organs in the male do not always, however, terminate in so favourable a manner. Professor Denonvilliers operated, September 1, 1853, in the Hôpital St. Louis, upon a young man suffering from both epispadias and an imperfectly-closed bladder. An attempt was made first to fill up the vesical opening with a flap of integument from the abdomen, and then to form an urethra by a portion of the scrotum; but, in a few days, symptoms of peritonitis supervened, and the patient died within a week of the performance of the operation.

Operation to restore to the Penis a Covering, the Skin having been destroyed by Disease. By M. Marchettini.—A syphilitic ulcer had destroyed all the skin of the penis, and a considerable portion of that of the pubes; the scrotum was entire. M Marchettini pinched up a fold of the scrotum, passed a knife through the centre, leaving the extremities attached, and then slipped the penis through it. Eight days afterwards, the skin was firmly adherent to the penis, which was, however, bound down by its new covering. Two incisions were made through the scrotum, from the root to the end of the organ, by which it became free, and cicatrization was complete in a few days.—*Gaz. Med. Ital.*

Upon Wounds of Arteries, and their Treatment. By Mr. Butcher.—The direction given in works on surgery to cut down to a wounded artery, and to put a ligature above and below the seat of injury, is good in itself, but not always practicable. When the internal carotid artery is wounded by a piece of tobacco pipe thrust through the mouth, the common carotid must be the vessel tied. A similar necessity may exist even in wounds of the extremities, as has been shown by Mr. Butcher, F.R.C.S.I., Surgeon to Mercer's Hospital. We find, in the 'Dublin Quarterly Journal of Medical Science,' an account of a wound of the profunda artery; ligature of the femoral, below the margin of Poupart's ligament, by a transverse incision; arrest of the bleeding; death eleven hours afterwards. It was impossible to tie the artery above and below the wound, owing to its great depth from the surface; the main trunk was, therefore, secured by a ligature, the incision through the skin being made transversely, as previously practised by Mr. Porter, in consequence of the necessity of putting on the ligature so close to the crural arch.

The most interesting part of Mr. Butcher's communication is the narration of those cases in which he commanded hæmorrhage from a large vessel by means of well-adapted pressure. A policeman was stabbed in the leg: severe arterial hæmorrhage, followed by faintness, ensued. Mr. Butcher concluded, from the

nature of the accident, that the posterior tibial artery was wounded. Graduated pressure, by means of a roller and compresses (7 or 8 pledgets over the situation of the wound) was carefully exerted; the foot was raised; a compress and roller were applied over the popliteal artery, and a dose of morphia was administered; the limb was kept steady by a splint. On the following day, the pressure was removed from the popliteal artery and applied to the femoral, by means of an aneurism compress on the groin. The case did well; and Mr. Butcher, in commenting upon it, remarks that, as a rule, the surgeon should not seek for a wounded artery unless it be bleeding. A case of wound of the ulnar artery above the wrist was successfully treated by compression of the wound and pressure over the brachial artery; and profuse hæmorrhage from the hand after excision of the index finger, with removal of the head of the metacarpal bone, was completely controlled by powerful flexure of the injured limb, together with gentle pressure over both the radial and the ulnar arteries.

Hydatid Cysts of the Liver treated by Puncture with a Capillary Trochar, and injected with Tincture of Iodine. By Dr. F. A. ARAN, Physician to St. Antoine Hospital.—Cysts in the liver, formed usually by a collection of acephocyst hydatids not uncommonly burst into organs, where the discharged fluids excite disturbance dangerous to life—e. g., in the lungs; hence various attempts have been made to open the cyst externally through the abdominal walls, by surgical operation. Recamier endeavoured to establish connexion between the cyst and the abdominal wall, by successive applications of caustic paste, that the fluid from the cyst might not, when discharged, find its way into the peritoneal cavity. Jobert de Lamballe introduced a large trochar (for the purpose of injecting iodine), which he allowed to remain in the wound. M. Aran uses capillary tubes, and he relates the particulars of a case,* in which, after ten successive punctures, the following fluid was injected—tinct. iodine, ℥ij.; aquæ destill., ℥ij.; potas. iodid., ʒiss. No pain ensued, and the cyst, which had previously contained thirty-two ounces of reddish fluid, slowly contracted. A second case also is recorded.

Large Sanguineous Cyst of the Thyroid Body successfully treated by Puncture and Injection of Tincture of Iodine. By HOLMES COOTE, F.R.C.S.—The cyst, which extended under the sterno-mastoid muscle to the anterior border of the trapezius, was developed in a healthy young married woman, shortly after her first confinement. It was thrice punctured. There first flowed out sixteen ounces of colourless blood, which separated into a firmish yellow clot and serum. Subsequently, the fluid was of pale-red hue, and much less in quantity. An ounce and a half of the following was injected through a trocar:—Tinct. iodine co., ℥j.; aquæ, ℥v. After the second injection the cyst disappeared entirely, without inflammatory symptoms. The patient is now well.† M. Nelaton has used a similar injection with success, for the cure of a biliary fistula, consequent upon the opening of an hydatid cyst by caustic potash. He also injected a spina bifida without any bad results. Bouchut has cured a chronic abscess (*abscés froid*) by the same means, in a young scrofulous patient, in the Hôpital Sainte Eugénie Dr. Giamb. Borelli‡ has used the injection equally successfully in three cases of ranula, a disease which, according to modern investigation, seems by no means invariably connected with dilatation of Wharton's duct. Its seat is probably, in many cases, a bursa first described by Fleischmann, and situated upon the outer surface of the genio-hyoglossus muscle under the tongue.

But it must not be inferred that iodine injections are wholly without risk. M. Nelaton§ relates the case of a young man suffering from Pott's disease—i. e., psoas abscess. The injection of tincture of iodine, one part; water, two parts, with as much iodide of potassium as was necessary to keep the iodine in solution,

* Revue Méd. Chir de Paris, huitième année, tome xvi.

† Medical Times and Gazette, Nov 4th, 1854. ‡ Gazz. Sarda, ii, 1854.

§ Journ. de Chem. Med., i. 1854.

was followed, five hours afterwards, by dizziness, disturbance of vision, sickness, coldness of the extremities, small pulse. On the following day, there was continued sickness, violet suffusion over the eyelids, and deep pain in the neck. Active measures were resorted to, in order to get the poison out of the system—namely, emetics and purgatives of croton oil. Sinapisms were applied to the feet. From these symptoms of poisoning the patient recovered, but the disease remained in the same state as before.

But inflammation, excited by any cause in a cyst, is commonly followed by serious consequences. It may ensue from a simple puncture. A young woman was admitted into St. Bartholomew's Hospital, April 21, 1853, under Mr. Lawrence, with a large cyst of the thyroid body, bound down by the sterno-mastoid muscle. A small trocar, introduced into the mesial line of the swelling, gave escape to six ounces of sparkling, brownish-red fluid, composed of discoloured blood and cholesterine. After a week, the fluid having re-accumulated, Mr. Lawrence re-opened the original wound, and introducing a director, cut open the front of the cyst with a bistoury. The same quantity of fluid escaped, darker coloured than the preceding, accompanied by some bubbles of fetid gas. Considerable constitutional disturbance followed this operation; there was redness of the neck, violent pulsation of the carotids, &c. A cure was effected by repeated injections of small quantities of the compound tincture of iodine.

On Cystic Tumours. By VERNEUIL, VELPEAU, MALGAIGNE, ROBIN, &c.—Three modes of origin are recognised for the development of cystic tumours—(1) by the enlargement of spaces in the areolar or other tissues of the body; (2) by the dilatation of natural ducts, sacculi, or bloodvessels; (3) by the growth of newly-formed elementary structures, having the characters of cells or nuclei. Of the first variety, specimens are familiar to all in the bursæ mucosæ, which form wherever two hard moveable substances come into contact in the body. Of the second, we have several additional illustrations in the recent records of surgery. Of the third, to which Rokitansky attaches no small importance, no considerable additional confirmatory evidence has been received. The hydatid testicle of Sir A. Cooper is shown by Mr. Curling to consist in dilatation of the seminal tubes.* Sir B. Brodie's statement of the origin of cystic disease of the mammary gland in dilatation of the lactiferous tubes has been confirmed by frequent dissections. Mr. Quekett's injections have satisfactorily proved that cystic degeneration of the kidneys commences in the Malpighian capsules, or the tubuli uriniferi. Mr. Coote has traced the origin of certain cystic tumours to dilated bloodvessels, and especially veins. Dr. Verneuil† has contributed the account of cyst formation in consequence of dilatation of the ducts of the sweat-glands. Cysts about the joints have been shown, not only by most English writers on surgery, but also by the French—namely, Velpeau, Malgaigne, Foucher, Bauchet, &c., to be derived either from some synovial bursa or sheath, connected with the tendons, or from some process of the synovial membrane. In skin, in mucous membrane, in gland-structures, in bloodvessels, and about joints, cystic tumours may be referred to the dilatation and enlargement of pre-existing tubes. In the 'Gazette Medicale de Paris' (Nos. 22 and 23, 1854), M. Robin classifies erectile tumours, according to the experience of many English surgeons, into—(1) tumours formed by the dilatation of capillaries, nævi materni; (2) tumours formed by the dilatation of veins; (3) tumours formed by the dilatation of arteries; (4) tumours formed by the extravasation of blood, around which a wall forms; or aneurisms by erosion. Rokitansky objects to the term "erectile tumours," as universal in such cases, assigning a different mode of origin, and substituting the name "cavernous tumours," as more accurately expressing their character. He states,‡ that the stroma of erectile tumours is formed of a network of fibres, analogous to those of cellular tissue. The thickness of the septa is very variable; from the thicker ones there radiate

* Med. Chir. Trans., xxxvi. † Gazette de Paris, 53, 1853.
‡ Zeitschrift der K. K. Gesellschaft der Aerzte zu Wien, von Hebra, 1854.

others, more delicate, which incompletely circumscribe irregular spaces, communicating one with another. In these varieties we find blood, either liquid or coagulated, or concretions resembling phlebolithes. The seat of these tumours is variable; but they are most commonly met with in the liver. Next in order of frequency, Rokitansky puts the subcutaneous cellular tissue, the skin; the face, the trunk, the limbs; the cranial bones, the dura mater, and the pia mater. The primitive element of development in these tumours consists, according to the Viennese professor, in trabeculæ (minute septa), sometimes transparent and smooth, sometimes slightly striated, in oblong nuclei, and in fusiform caudate cells. From these trabecules extend processes which bound irregular spaces, in which blood-vessels become developed. He affirms that the independence of these tumours from surrounding vessels at the commencement of their formation is illustrated by their development in the liver, where it will be found that the blood does not enter them, except by the perforation of their areoles. We cannot say that in our opinion these views, however elaborately worked out and cleverly expressed, are yet substantiated On the contrary, the immediate dependence of such tumours upon bloodvessels seems to receive, from dissection and examinations, yet further corroboration.

The dependence of cystic degeneration of the kidney upon dilatation of the Malpighian capsules and uriniferous tubules has received confirmation from an observation of MM. Guilleton and L. Ollier, entitled, 'Upon the Abnormal Development of the two Kidneys, in a Fœtus, offering an Obstacle to Accouchement.'* The patient, æt. 33, the mother of four children, had reached the full term of her fifth pregnancy without the occurrence of any circumstance worthy of note, except that the abdomen was much more prominent than usual. July 1. She felt something slip into the vagina, while making a violent effort at stool; it proved to be a foot-presentation. Manual efforts to extract the child were unavailing, and the patient was conveyed to the Charité Hospital, where delivery took place, by the contractions of the uterus, and the patient recovered, although the case was complicated with an attack of metro-peritonitis.

The abdomen of the fœtus, of great size, seemed, when opened, to be entirely occupied by two enormous tumours, extending from the sides to the mesial line. They were the kidneys enormously enlarged, and containing a multitude of cysts. Microscopical examination showed that the tumours contained, as principal elements, uriniferous tubules and closed vesicles; and the authors consider, after noting the relations of the vesicles with the vascular element of the organ, and the disposition in the middle of the cortical substance, that the hydatiform element belonged to the glomerules of Malpighi. Thus the morbid change consisted in hypertrophy, of hydatiform character, of the glandular substance, without the formation of any new product.

A cavernous network of veins has been described by Kohlransch† on the alæ nasi. The fact has been known some time, but it is mentioned (and that but briefly) only in Hyrtl's 'Anatomy.' This disposition of the vessels may be verified with facility by the injection of air. If such a preparation, so injected, be first hardened by immersion in spirit, and then cut with a sharp knife, this vascular structure can be readily distinguished. It lies between the periosteum and the mucous membrane, the vascular meshes perpendicular to the bones. The mucous glands, which, in other parts of the Schneiderian membrane, are superficial, with short infundibuliform ducts, are here placed deeply, and with longer ducts. This arrangement explains the sudden swelling to which the mucous membrane of the nose is subject; the considerable excretion of liquid so frequent in this condition; also the frequent hæmorrhages from the organ. In this situation mother-spots or nævi sometimes form, and extend with great rapidity. Their destruction by strong acids is unfortunately attended with deformity to the face, unless they are completely removed at an early period when small.

* Gazette Médicale de Lyon, Août 31, 1853.
† Müller's Archiv, No. 2, 1853, p. 149.

The Radical Cure of Hernia. By Velpeau, Maisonneuve, Jobert, Wützer, &c.—Attempts are still being made to effect by operation the permanent obliteration of the canal down which a hernial protrusion has occurred. In 1837, M. Velpeau demonstrated the possibility of obtaining a radical cure by means of iodine injections; but the difficulty of introducing the instrument into the hernial sac was sufficient to deter surgeons from following this suggestion. Seventeen years afterwards, M. Jobert made some new trials, and with satisfactory results, but the means of execution remained precisely the same. M. Maisonneuve, surgeon of La Pitié, now proposes a new and simple method of making the fluid penetrate the hernial sac, without danger of its passing into other parts. Supposing the case to be one of scrotal hernia, the viscera are first returned into the abdomen; M. Maisonneuve then seizes the middle part of the scrotum between the thumb and index finger of the left hand, opposite to where lies the empty hernial sac; he pierces it through with a long and thin trocar up to the hilt, and then withdraws the perforator, leaving the canula in the wound. The skin of the scrotum is then drawn over the canula in such a way that two openings, that of entrance and that of exit, should be separated as widely as possible. Lastly, the canula is slowly withdrawn, and when its point is fairly within the sac, the fact is known by its great mobility. This proceeding, somewhat complicated in description, is simple as possible to the operator.—*L'Union Médicale*, Oct. 21, 1854.

The operation of Professor Wützer, of Bonn, has been successfully practised by Mr. Spencer Wells, the particulars of whose cases are mentioned in the 'Medico-Chirurgical Transactions,' vol. xxxvii. The funnel of integument is derived from the scrotum, as in the preceding operation, but it is kept in situ by a wooden instrument or plug, through which runs a long curved needle, by which the funnel is, as it were, "pegged" to the abdominal walls. We consider this operation preferable to that suggested by Maisonneuve, who introduces his finger as far as the plug of integument will allow, and then cutting down upon the tip, dividing all parts composing the abdominal walls, even the peritoneum, attaches the plug to the sides of the wound by sutures.

On the Value of the Treatment of Blennorrhagic Orchitis by Collodion. By M. Ricord.—Thirty-eight patients have been treated, under M. Ricord, by elastic collodion, according to the rules established by M. Bonnafont. The pain produced by the application of this substance lasted from six minutes to a quarter of an hour. The greater number of patients felt easy from that time, but others experienced a recurrence during the course of the day. One of them complained of most acute suffering. The inflammatory pain diminished in some, and ceased in others, without any sensible influence from the collodion; in a certain number of cases the pain was sensibly augmented. In none of the patients treated in this way had M. Ricord to relate the miraculously rapid cure announced by M. Bonnafont. After twenty-four hours the tumour had not sensibly diminished in volume; it presented, in the majority of cases, the diminution of one-third of an inch after forty-eight hours; then the diminution continued to operate gradually and progressively. The mean duration of the malady in the cases treated by collodion was seventeen to eighteen days.

At the same time, eleven patients, also affected by blennorrhagic epididymitis, were treated by repose, strict regimen, and topical cold. The inflammatory pain disappeared promptly, and the diameter of the tumour was sensibly diminished at the end of thirty-two hours. The mean duration of the malady was fifteen to eighteen days. A third series of patients was treated by compression by strapping. Here the symptoms disappeared more rapidly than in the preceding plans. The mean duration of treatment was fifteen days. One patient was discharged cured in six days. M. Ricord believes that elastic collodion does not cause such suffering as the ordinary collodion, but that it is a more painful mode of treatment than others. M. Velpeau believes that collodion does not shorten the duration of orchitis; it sometimes diminishes the pain; on other occasions it

increases it; it irritates the skin; often excites suppurations, which are tedious and inconvenient. M. Bonnafont has explained his success upon the fact of his having treated his cases at the very outset, a circumstance quite possible in a military hospital.

There is in this general account a confusion which M. Malgaigne has pointed out, represented too often in statistics destined to prove the value of certain medicines. There should be taken into consideration the care which one takes of the disease at its outset; also whether the malady is of more or less standing. M. Velpeau and M. Ricord arrived at a mean duration of eighteen days; but there are many instances of orchitis cured in a much shorter space of time; others which last much longer. Are there not in some of the cases instances of the rapid natural cessation of the disease? M. Bonnafont's report should be compared with another, in which a series of precisely similar, i.e., recent, cases were treated by other surgeons; for when patients arrive at an hospital with swelled testicle, the symptoms aggravated by six or eight days' neglect, the symptoms are necessarily more severe. M. Velpeau, analysing seven observations, published by M. Armand, of orchitis treated by collodion, found in one a duration of ten days; in another, thirty-three; and taking the whole number, a mean of twenty-two days and a half, dating from the commencement of the attack. M. Bonnafont, dating his cases from the commencement of the treatment, reduces the mean duration to twelve days. Had the orchitis been taken by both surgeons at the outset of the disease, they would, in all probability, have arrived at the same result. Is it rational to attribute to the treatment a duration which does not belong to it, supposing, for example, that it happened not to be applied until the twentieth day? On the other hand, if the collodion is applied when inflammatory swelling is subsiding, and only two or three days are required for the spontaneous cure, can one say that the patient owes his recovery to collodion? From this disorder, both in ideas and logic, the numerous facts brought together are so badly arranged, that it is impossible to arrive at any definite conclusion.—*Revue Medico-Chirurgicale, Oct*, 1854.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)
Physician to the Metropolitan Free Hospital.

I. Diseases of Women in the Unimpregnated State.

1. *On the Treatment of Prolapsus of the Uterus, and especially by* Zwancke's *Pessary.* By Professor Chiari. (Zeitsch. der K. K. Gesell. der Aertze zu Wien. June, 1854.)
2. *On the Snout-shaped and Polypoid Elongations of the Os Uteri.* By Rudolph Virchow. (Archiv für Pathol. Anat. Band vii. Heft 1. 1854.)
3. *A New Method of Dilating the Os Uteri.* By Dr. Carl Braun. (Klinik. der Geburtsh.)
4. *A Case of Puncture of the Ovaries per Vaginam.* By Dr. J. Schnetter, of New York. (Verhandl. der Phys. Med. Gesell. in Würzburg. Band v. Heft 1. 1854.)

1. Professor Chiari speaks favourably of a form of uterine pessary contrived by Zwancke, of Hamburg. This instrument consists of two spoon-shaped blades, which are prolonged below into simple stems, and joined together by a Charrier's hinge, so that by means of a screw the two blades may be made to diverge from each other. It is introduced with the blades in opposition: their separation when inserted expands the upper part of the vaginal canal, and supports the uterus. Chiari says it is very efficacious, easily worn, gives no pain, and occasions no

hindrance to walking. He has used it with most successful results in many cases.

2. Virchow draws attention to a particular form of hypertrophy of the vaginal portion of the uterus, which consists in enlargement and elongation so as to resemble a hog's snout, and which Ricord has called the *col tapiroide*. Kennedy, Malgaigne (and we add Montgomery,) have observed that this enlargement has been mistaken for polypus, and removed. Some of the cases observed by Virchow were of a character to give rise to a similar mistake. He assigns the origin of this form of elongation to an abnormal development and projection, or eversion, of the mucous membrane of the cervical canal.

3. Dr. Braun designates the instrument which he has contrived for the dilatation of the os uteri the *colpeurynter*. It consists of a vulcanized india-rubber bladder, from two to four inches in diameter, and four inches in length, with an india-rubber tube enclosed in horn, fitted with a brass stop-cock, and a ring through which to pass a silk belt. When used, the india-rubber bladder is introduced empty into the vagina, then gradually distended by injecting cold or warm water. It is retained in situ by the belt fastened round one or both thighs or hip. The horn cylinder is curved in the direction of the pelvic axis: it allows only the upper end of the vagina to be stretched by the bladder; and obviates any unnecessary pressure upon the urethra or external parts. The operation is called colpeurysis. The advantages of this proceeding over other methods of dilating the os uteri, whatever the indication for that operation, are highly extolled.

4. The case of Dr. Schnetter is one of double ovarian dropsy, two cysts having been successively punctured per vaginam. The patient was twenty-five years old; she had been delivered by the forceps, after a difficult labour, a year before she came under Dr. Schnetter's care; she had suffered from painful menstruation before pregnancy; and during pregnancy, from unusual distension of the abdomen and difficult respiration. Fever and marked symptoms of peritonitis followed delivery. Examination revealed a tumour reaching a few inches above the umbilicus, occupying the right hypogastrium. The os uteri was in its normal position. To the right, somewhat behind it, and a little higher, a round and but slightly elastic tumour was felt. The uterus was longer than natural, and bent forwards towards the pubis. The continuity of the tumour in the abdomen with that in the pelvis was ascertained by counter pressure. A curved trocar was pushed into the tumour, when it projected behind the cervix uteri. At first, blood, then a few ounces of discoloured pus followed. The trocar was driven further in, and by means of a curved knife passed through the canula, a further opening into the tumour made. A little bloody serum followed. A tube was adapted to the wound. Considerable fever and pain ensued. Some days after the operation, pressure being made on the tumour through the abdomen, about four or five pints of offensive purulent gelatinous matter, mixed with fibrinous shreds, escaped through the tube. The tumour disappeared. Discharge continued for some time. The patient's health did not improve. Four months after the first puncture, examined again: another tumour was found projecting into the vagina on the *left* side. This cyst was punctured in like manner. Fever and alarming inflammatory symptoms followed. Six days afterwards an abundant discharge took place, gelatinous and fibrinous in character. The wound healed six months after the puncture. The patient eventually recovered her health; and eighteen months after operation there was no sign of relapse.

II. Labour.

1. *On the Rising and Falling of the Pulse during Labour-pains.* By Professor Martin, of Jena. (Arch. f. Phys. Heilkunde, 13th year, 3rd part, 1854.)
2. *On the Regularly-rising and Falling Frequency of the Pulse during Labour-pains.* By Friedrich Mauer. (Same work as preceding article.)
3. *Cases of Cæsarean Section.* By Bagenstechen and Ed. Behm. (Schmidt's Jahrb. der Ges. Med., 1854.)
4. *Cæsarean Operation on account of Pelvic Tumour.* By M. Retzius. (Schmidt's Jahrb. d. Ges. Med., 1854.)
5. *A Case of Cæsarean Section, preceded by a Summary of Three other Cases observed in the Maternity at Turin.* By M. S. Giordane. (Giornale delle Sc. della R. Accademia Méd. Chir., di Torino. July, 1854.)

1. The observations of Professor Martin and of Herr Mauer on the influence of labour-pains upon the pulse are of great physiological and pathological interest. Professor Martin refers to the observations of Hohl in 1834 ('Geburtshulfliche Exploration'), that, with every uterine contraction, the uterine-rush became more frequent, louder, and accompanied by a peculiar singing, shrieking tone, until it ceased at the acme of the pain, returning, however, in similar frequency, and then gradually subsiding to what it was before the pain. Professor Martin discriminates between the cause of the regular rise and fall of the uterine-rush, and the cause of the peculiar singing, shrieking tone which accompanies the uterine pulsation during the pains.

The rising and falling frequency of the pulse is to be sought for in increased action of the heart, which may be perceived by the touch applied to the arteries. He therefore studied this by feeling the radial artery of women in labour. He did not, however, observe the cessation of the pulse at the acme of the pains. There might be some special conditions in the uterus to cause it there. He noticed the frequency of the pulse of a parturient woman, which, in the intervals between the pains, beat from 5 to 6 times every five seconds. It rose at the beginning of every pain to 7, and gradually even to 8 and 9, and fell gradually back, somewhat in the following manner, from 5 seconds to 5 seconds: 5 5 6 6 7 7 7 8 8 7 8 8 | 7 7 6 5. When the labour was more advanced and stronger, the pulse in the intervals continued increased in frequency until the expulsion of the child, so that it fell back after the pains to 7, 8, or 9 beats. The following scheme was then observed: 7 7 8 9 9 9 10 11 11 12 12 13 | 12 13 13 13 14 13 13 12 1[illegible] 12 11 11 | 11 10 10 9 9 8 8 8 7.

Similar results were obtained from observations repeated, under varying conditions, upon many different women

The professor points out the following applications: 1. A new aspect of the condition of contraction; 2. An new phenomenon whereby to judge of the onset, duration, and strength of the pains; 3. A new and very desirable symptom in the discovery of failing labour-pains. He argues, that the nervous centre of the uterine contraction is the sympathetic; and that the heart's action is influenced by the uterine contractions through the sympathetic nervous system.

2. The observations of Herr Mauer were, for the most part, made in the clinique of Professor Martin. They were carried out with greater minuteness than the professor's. The results are confirmatory. Following Naegele, Busch, and others, he divides labour into five periods; and observes the influence of the pains upon the pulse during each. In the first period, the frequency of the pulse rises, at most, but 1 or 2 beats in 5 seconds above the normal rate of the intervals. In the second period, the frequency rises higher, possibly from 7 to 10 and 11 beats. This rising in the frequency of the pulse becomes more and more marked in the later periods; and in the fourth period it is the most conspicuous. In the fifth (the placental) stage, the rising is small, not more than 2 or 3 beats in the 5 seconds.

These constant relations are not observed in defective uterine contractions, or in false pains. The screams and groans of parturient women do not appear to affect the rising and falling of the pulse. Chloroform does not lower the pulse of women in labour, as it does that of persons preparing for surgical operations.

As to the question, What is the cause of the rising and falling of the pulse during a pain? Is it—1. Muscular action; 2. Accelerated respiration; 3. Direct stimulation through the blood; 4. Irritation in the nerve-centres, which influences the heart's action? After eliminating the first three conditions, he, like Professor Martin, declares for the influence of the sympathetic nervous system.

(The reporter ventures to submit that, whilst a certain influence may be accorded to nervous action, another explanation may be given. In the intervals between pains, a large proportion of the mother's blood is freely circulating in the uterus and placenta. On contraction supervening, the muscular compression cuts off, more or less completely, this proportion from its uterine diverticular course: it is thrown back into the ordinary circulatory apparatus, the increased volume of blood operates as an increased stimulus to the heart—hence the increased frequency of the pulse; a frequency, be it observed, which the observations of Professor Martin and Herr Mauer show to keep pace exactly with the increase of the uterine contraction, the pulse being quickest exactly at the moment of the acme of the uterine contraction. It is also interesting to note that the mother's pulse and the fœtal pulse bear an inverse relation, in point of frequency, to each other. The reporter has related, in another part of this journal, observations which show that the fœtal pulse is at the highest in the intervals of the pains—i.e., during the period of free intercourse between the maternal and fœtal bloods, and that it falls remarkably during the contractions of the uterus, when that intercourse is suspended.)

3. The first case of Cæsarean section of Pagenstecher was in a woman, 33 years old, pregnant a fourth time. There was osteomalacia of pelvis in a previously rachitic subject. The promontory of sacrum projected forwards, so as to have a conjugate diameter of from 2 to 2½ inches. The infant was baptized in the presence of the mother, eight weeks after the operation.

The second case is related by Behm. The operation was called for by an enormous bony tumour in pelvis. The patient was a delicate woman, aged 29, who had shown signs of rickets in youth. She had been delivered in first pregnancy by craniotomy; a tumour the size of a hen's egg, seated at the left sacro-iliac synchondrosis, then obstructing the birth. The rapid growth of the tumour was afterwards observed, till it filled nearly three-fourths of the true pelvis. Pregnant again in 1851, delivery by pelvis was impossible. Cæsarean operation was performed under influence of chloroform. The uterus contained two children, each having its own placenta. Blood escaped into peritoneum; and the patient died on the third day.

4. Retzius' case is that of a woman who, in a first pregnancy, gave birth to a living child with difficulty, on account of a tumour in pelvis. In her second pregnancy, the tumour was felt compressible, not painful, and so large as to prevent both the natural passage of fœtus and the use of instruments. The operation was performed under chloroform. The child was in incipient decomposition. The patient died forty-six hours after operation. The tumour arose from the margin of the foramen ovale. It was of an irregular round form, and of purely fibrinous texture.

5. The Cæsarean section has been performed during life *four* times within sixteen years, at the Maternity of Turin. During that time, about 1200 women have been admitted. In *eleven* cases, the section has been performed after death: in none of these did the child exhibit signs of life. The four operations during life was necessitated by faulty conformation of the pelvis. In the first 3 cases, the death of the mother followed within three or four days, and the autopsy exhibited effu-

sions, inflammation of the peritoneum, traces of pelvi-uterine phlebitis; and in 1, a hernia of the intestine through the wound in the uterus. In all the cases, the child was extracted living and viable. The fourth case is related by the author in detail. The subject was a primipara, aged 19, who had shown symptoms of rickets. All her six brothers, and all her five sisters, had died in infancy of scrofulous affections. She herself, while a child, had had both femora fractured by a carriage passing over her. To this she attributed her deformity. The vertebral column presented two lateral curvatures; but the lumbar region was not depressed. The left iliac crista was one centimetre higher than the right. The sacrum was not exactly central, deviating to the left. The sacro-pubic diameter, measured successively by the finger, and the pelvimeters of Baudelocque and Van Heuvel, gave different results: its extent was estimated at 2¼ inches; the cavity and outlet appeared about normal. The operation was performed under anæsthesia. From the second to the sixteenth day, intense symptoms of peritonitis were combated by bleeding and leeches. At the end of some days, a large quantity of blood, mixed with pus, flowed from the inferior angle of the wound. On the nineteenth day, the patient was improving; there was no fever, and occasional diarrhœa was easily stopped. The upper half of the wound was healed; the wound of the uterus, also, seen through the inferior part of the external wound, was also in process of healing. On the twentieth day, pains in the loins; the left leg slightly swelled at the ankles; the abdomen still painless. On the twenty-sixth day, the patient sank from phlegmasia dolens. The autopsy revealed a large quantity of flocculent pus in the abdomen and pelvis, and recent adhesions of the peritoneum. The venæ cavæ et iliacæ were healthy; but the left femoral had its walls thickened, infiltrated, and was filled with coagulated blood and pus.

The prepared pelvis was an excellent type of the pelvis obliquè ovata of Naegele, which explains the discordant results of the mensuration of the sacro-pubic diameter employed.

QUARTERLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, &c.

By W. B. Kesteven, F.R.C.S.,
Member of the Council of the Epidemiological Society.

I. Injuries, &c.

Rupture of the Bladder.—In the month of September last, a young woman died at Portsmouth, whose death was supposed to have been caused by violence. A feeling of indignation was excited by some reports which the daily papers circulated against the officers of a certain ship. On a strict and very protracted examination it was shown, from the medical as well as other evidence, that the girl had died from peritonitis, caused by rupture of the bladder; and that this injury had proceeded from falls, concurring with an over-distended bladder, the deceased having been at the time intoxicated.

Case where it was doubtful whether Death was caused by Drowning, Apoplexy, or Violence.—On the 13th of June, the body of a male was taken out of the Rhine, and an investigation instituted as to the cause of death. The body was completely clad, and was that of a strongly-built youth, about sixteen or seventeen years of age, and presented no trace of decomposition; the integuments of the hands and feet were slightly wrinkled; it was probable that the body had not lain in the water more than three days. The face was swollen and blue. The pupils were dilated; a greenish fluid flowed from the nostrils; the mouth was firmly closed; the tongue was found behind the teeth; the abdominal parietes retracted; there was no *cutis anserina* apparent. On both of the temples were traces of former leech-bites, about the middle of the forehead, extending up into the hairy

scalp, was an oblong space of about six square inches in extent, where the integuments were of a reddish-brown colour, and exhibited distinct appearance of extravasation of blood, which on dissection was found beneath the inguments, and under the aponeurosis. The bones of the skull were uninjured. The *pia mater* was much loaded with blood, as were also the other vessels of the anterior lobes of the brain. Soft plastic lymph was found beneath the arachnoid. The larynx was open; no water found in this or in the trachea. The lungs were moderately distended and crepitant. The left lung was firmly adherent to the pleura. The heart was rather empty of blood. The abdominal viscera were healthy, except that the liver was enlarged.

The physician by whom the dissection was performed concluded that death had been caused in this case by suffocation, as he attached considerable weight to the fact of there having been no water found in the trachea, although the glottis was patent. It was doubtful whether the extravasation of blood within the cranium was the result of apoplexy, suffocation, or violence; the exudation of plastic lymph showed some degree of reaction during life. It was evident, therefore, that the contusion observed on the forehead had been inflicted during life; the interval that had occurred between the infliction of this injury and the submersion of the body was uncertain. The effusion of plastic lymph sometimes takes place with great rapidity. Neumann relates that he had noticed firm adhesion of the intestines to have taken place when life had lasted only twenty minutes after the receipt of injury. Positive evidence was wanting to determine whether death had occurred from the cerebral lesion, after or before submersion.

Dr. Simeon, of Maintz, to whom the preceding facts and opinions were submitted, is of opinion that such an amount of extravasation and effusion is not likely to have occurred from violence externally applied, so limited to one region, without any injury to the bony structures; neither does he deem it probable that such an extent of exudation of plastic lymph can have been produced in a very short time, as the result of concussion of the brain, or of apoplexy; pathology teaches us that it must have been the result of inflammation, localised to the anterior lobes. That such a malady had existed was likewise to be inferred from the presence of the cicatrices of leech-bites on the temples.

The conclusion of Dr. Simeon was, that the deceased, who was a sailor, and had been drinking, had fallen overboard from his ship as it lay at anchor, and had struck his head in falling, so that he was completely stunned, and thereby disabled from struggling.—*Casper's Vierteljahrschrift*, July.

Survivorship in Drowning.—In our last report we had occasion to discuss this subject with reference to the case of Underwood *v.* Wing, in which a decision contrary to every probability was given. We dwelt upon the circumstance, that in the case of a husband and wife being submerged at the same moment, the wife, by reason of her relative physical feebleness, would be drowned before her husband. That view has received confirmation from the painful narrative of one of the survivors of the awful catastrophe that befel the Arctic steamship, which sank in consequence of a collision at sea. Out of four hundred and forty souls, not one hundred escaped death by drowning. One of the few survivors, named McCabe, clung to a raft for two days, and saw at least seventy individuals, who had with him taken refuge thereon, drop one after the other exhausted into their watery graves. "The women," he observed, "*were the first to go; they were unable to stand the exposure more than three or four hours.* They fell off the raft without speaking a word." After eighteen hours, there were not more than three or four individuals left on the raft with him. The rest of this simple but melancholy tale was that he remained the solitary being out of seventy. On the approach of the night of the second day, during which time he had neither eaten nor drunk, he was on the point of falling himself from exhaustion, when he was rescued by a ship's boat.

Condition of the Remains of a Man after Submersion in Water during Twenty-six Years.—In the year 1828 a miner, named John Stephens, aged twenty-four years, was working in the Penandrea mine (near Redruth, Cornwall), when he fell into the water-shaft; every exertion to extricate him was made in vain; and though diligent search was made for two months, the body could not be found. The shaft remained closed until April last, when, on the working of this mine having been resumed, the remains of the body of the deceased were found, where they had lain under thirty fathoms of water during the twenty-six years.

We are indebted to Henry Harris, Esq, of Redruth, surgeon, for a description of the condition of the bones, &c., as examined by himself and his three pupils: The bones were entire, but completely denuded of all trace of muscle, tendon, ligament, or cartilage; they had a dirty brown or blackish colour. On passing the finger into the cranium, it was found to be nearly full of a dirty brownish substance, of the consistency of very soft butter. This matter had not any odour, it appeared to consist chiefly of brain mixed with some mud. It was not, however, examined microscopically or chemically.

II. Medico-legal Osteology.

Can the Osseous Remains of a Corpse throw light upon the Age of Deceased?—Can the period that has elapsed since Death be determined by the Condition of the Bones? —Dr. Kanzler, of Delitzsch, has endeavoured to elucidate these two questions, by reference to facts recorded in medico-legal and other writings, and by the statement of his own observations. In attempting an answer to the first, Dr. Kanzler consulted thirty-seven authors, and has collated and condensed their descriptions of the development, growth, and characters of the bones, from their first appearance down to old age. The result of this labour is an article covering nearly sixty octavo pages, consisting entirely of detailed descriptions. These do not admit of abridgment within the space assigned to our report. We must, consequently, confine ourselves to giving an abstract of Dr. Kanzler's exposition of the second point: the determination of the period elapsed since death, as inferred from the condition of the osseous system. The difficulty of determining this question, even where the soft parts remain, is shown in the following instances. A body which had lain in a light gravelly soil had become so completely decomposed at the end of eleven months, that the bones were only loosely held together. The body of a child in the same ground was putrified, and the soft parts destroyed, in six weeks The soft parts of a body suspended in the open air during five days in the summer season had become decomposed. The body of a man, which had lain nearly two years and a half in a damp sandy soil, was exhumed on suspicion of murder, and was found in a tolerably good state of preservation; the odour of the body was mouldy rather than stinking, the muscular substance was pulpy, but its appearance was preserved; the countenance was recognised by those who had known the deceased in life. In another instance, a corpse was disinterred from ground which had not been disturbed for upwards of ten years, and was found not to have undergone decomposition, but had simply become shrunken and dry, the neighbouring corpses having undergone decomposition. Chemical analysis in this case did not discover the presence of arsenic to account for the preservation of the body.

Dr. Kanzler notes, from his experiments and researches, the following circumstances as modifying the rapidity and course of putrefaction: 1. *Age.* The bodies of infants putrefy more rapidly than those of adults. The soft substance of a fœtus in the second month will liquefy in the open air, or in moist ground, without the usual process of putrefaction. The same disposition is distinctly observable until the fifth month. 2. *Sex.* Putrefaction generally takes place more rapidly in females than in males. 3. *Obesity* favours decomposition. 4. *The nature of the previous malady.* The progress of putrefaction is more rapid after acute diseases than

chronic diseases, after exanthematous fevers, all diseases attended by sudden annihilation of the sensibility—e.g., apoplexy, lightning-stroke, &c. The bodies of those who have died from hæmorrhage undergo putrefaction slowly. 5. *External pressure.* The deeper the grave, or the higher the superincumbent mound, the slower the rate of decomposition. 6. *Clothing of the corpse.* Unclothed corpses decompose more rapidly than clothed, as also corpses enclosed in coffins of hard wood decompose more slowly than in coffins made of soft wood. 7. *Occupation.* So far as the influence of trade or occupation is known, it is stated that the bodies of chimney-sweepers and tanners putrefy slowly. (?) 8. The *Medium* surrounding the body. Orfila states that putrefaction takes place more rapidly in the open air, or in dung, than in water, more rapidly in the latter than in the ground, more rapidly in soils abounding in vegetable matters than in clayey moist graves; in the latter more quickly than in dry sandy graves. 9. *Temperature.* Decomposition is favoured by moisture, with warmth between 18° and 25° R. (=72° and 88° Fah.) In higher temperatures, especially without moisture, as in the deserts of Asia and Africa, the body becomes mummified. At low temperatures a body is preserved by congelation, as in the case of the corpse of Prince Menzikoff, who had been banished to Siberia by Peter the Great, which was found ninety-two years afterwards in a state of complete preservation. It has also been observed, that the corpses of those who have been interred at the Monastery of St. Bernard, in the Alps, 7200 feet above the level of the sea, have been preserved during many years by the extreme cold. 10. *The deposition of Ova* by insects upon the corpse accelerates putrefaction. [An instance of this kind was given in our last report.]

The changes which will be found to have taken place in bones during any period after interment, will necessarily vary very greatly with all the circumstances above-mentioned, as affecting the process of putrefaction in soft parts.

Dr. Kanzler gives the following general approximative statement. In from two to three years all the soft parts are destroyed; in from five to ten years the cartilages are partially destroyed, and scarcely any trace of medulla remains; after from ten to fifteen years the bones are greasy on their inner surfaces, and at the epiphyses; after thirty years the skeleton is rarely found perfect, only the larger bones remaining; after seventy or eighty years, the thigh bones and base of skull only will be found; after eighty or a hundred years these bones are light, brittle, and porous.

Dr. Kanzler quotes the statements of several authors confirming the preceding observations, and at the same time mentions numerous exceptions of bones having been found entire after many hundreds of years' interment.—*Casper's Viertel-jahrschrift*, July.

To the above we take the opportunity of adding Dr. Casper's observations upon the successive decomposition of internal organs, as given in his *Gerichtliche Leichenoffnungen:*

1. The internal organ in which putrefaction is earliest observable is the trachea and larynx, in which the change will be found to have commenced almost as soon as green spots have appeared upon the walls of the abdomen. When the abdominal parietes are entirely green, the mucous membrane of the trachea, although all other internal organs may be still intact, will be found of a dark cherry-red colour, which gradually becomes of a dark reddish-brown. Casper states that he has found only one doubtful exception out of many hundred dissections.

2. The brains of new-born children, and of infants under two years of age, come next in order of time, being quickly converted into a thin rose-tinted pulp.

3. The stomach resists putrefaction longer than the preceding, but yields sooner than other internal organs. The first traces of its decomposition consist in detached, dirty red, ill-defined, and irregularly-shaped spots of various sizes in its fundus, where, usually, bluish-red streaks of the veins are found, and which show through these reddish spots. It is of importance to bear these in mind in cases of suspected poisoning. As putrefaction advances these spots enlarge, until the whole mucous surface presents the same condition. Dr. Casper has never seen,

as the result of putrefaction, the entire separation of the mucous membrane, sometimes seen in poisoning with corrosive sublimate.

4. The intestines next undergo a change similar to what has been described in the stomach. In extreme putrefaction of the body, these organs become converted into a dark green pulp, without trace of their structure.

5. The spleen will sometimes be found to be in a state of incipient decomposition earlier than the stomach and intestines, although most frequently it follows them in order.

6. Omentum and mesentery; the fatter these organs, the more rapidly they undergo putrefaction.

7. The liver is often found fresh, and of its normal colour, many weeks after death. Putrefaction commences on its convex surface. The first change of colour is greenish, which extends over the whole, changing to a grey and finally to black, the parenchyma softening as putrefaction advances.

8. The brain of adults; the first traces of putrefaction are found on its lower surface, a light greenish discoloration, gradually spreading over the cortical, and extending into the medullary substance. After several months, the cerebral matter is changed into a reddish pulp, similar to what is early observed in the bodies of infants

9. The heart may be found entire when stomach, intestines, &c., have become putrid. In this organ, first the columnæ carneæ, then the walls, and the entire organ, become softened, greenish, and lastly of a grey colour.

10. The lungs begin to exhibit putrefactive changes about the same time, or occasionally a little earlier than the heart. The lungs are often found unaltered when putrefaction is far advanced in other parts. This fact, Dr. Casper observes, furnishes an argument against the inference sometimes drawn from the hydrostatic test to the lungs of newly-born infants, that their floating is referrible to putrefaction. The first traces of decomposition in the lungs is seen in the formation of gaseous vesicles beneath the pleura, at first single, then increasing and multiplying until an entire lobe is thickly studded therewith; these are more especially observable on the lower lobes. In the further progress of putrefaction the substance of the lungs becomes softened and dark-coloured, lastly black and pulpy.

11. Kidneys. 12. Urinary bladder. 13. Œsophagus. 14. Pancreas. 15. Uterus, last of all to putrefy.

Extraordinary Strangulation of a Child by its Mother whilst in a Dream.—Yesterday, Mr. William Payne held an inquest at St. George's workhouse, Mint-street, Southwark, on the body of Henry Rushton, infant son of Mr. James Rushton, of Little Rodney-street, Suffolk-street.

James Rushton, father of the child, said that he went to bed between one and two o'clock on Sunday morning last. The deceased child slept in the same bed with its mother, who, at the time he went to bed, had it in her arms. He heard nothing of the child during the night, but about seven o'clock in the morning, he was awakened by his wife, who in alarm called him by his name. He inquired what was the matter? and she said that the child was quite cold. He immediately got a light, and then saw that the child was black all over the face. Soon afterwards he went for Mr. Llewellyn, the surgeon, who came directly, and he said that the child was dead. His wife told him that she had had a fearful dream.

Mr. William Llewellyn, of No. 12, Great Suffolk-street, said he was called on Sunday morning to the house in question, and found the child dead. It had been so for some time. The tongue protruded, and the face was very livid. It had all the appearance of having been strangled. He questioned the mother, and she told him she had dreamt that a mad bull was attacking her, and had squeezed up the child to protect it, and when she awoke, as she found the child cold, she called her husband. The child had been properly taken care of. The parents had three other children, were very industrious people, and kind to their children.

The child was lying on her arm, and its death might, very probably, have occurred as she described it.

The jury returned a verdict of Accidental Death.—*Morning Herald*, Nov. 23.

Live Birth at Four Months.—Mrs. R. menstruated on the 8th Feb., and quickened 8th June. On the 17th June, a fœtus was expelled, which weighed exactly nine and a half ounces, and measured eight inches in length; its placenta weighed six ounces. The eyelids were adherent, the nose and mouth closed, the membrana pupillaris entire. The lungs, in colour and volume, resembled those of an early fœtus, and, with the exception of one or two ecchymosed spots, no colour or other evidences of developed air-cells were noticed, all the appearances indicating that no air whatever had reached the tissue of the lungs.

The pulsations of cord, which were vigorous, were allowed to continue for some time, in order that the reflex movements of the limbs, face, and respiratory muscles might be observed. On touching the hands or feet, or blowing upon the face, a convulsive movement of the limbs or respiratory muscles followed. When the pulsations of the cord had fallen to ninety beats, it was divided, and about a drachm of blood suffered to escape; the heart's action immediately became quicker, and one or two thoracic convulsions followed. The reflex movements gradually became more feeble, and ceased in about an hour.

Dr. Keiller, who related this case to the Edinburgh Obstetrical Society, pointed out the medico-legal relations of this case. They are obviously important.—*Edinburgh Monthly Journal*, September.

III. Toxicology.

Poisoning with Arsenic.—Dr. Blondlot, of Nancy, has related the particulars of a series of four murders, in which arsenic, administered by one individual, was detected in bodies that had been buried sixteen and twenty years. In the latter instance, the coffin had become entirely disintegrated, and the bones of the skeleton lay detached from each other, the ligamentous parts having disappeared. The brain, however, was found entire, but shrunk to the size of a fist. The weight of this brain was about eleven ounces. When very carefully analysed, it was found to contain arsenic. The presence of arsenic in the earth of the cemetery was excluded by careful analysis.—*Journal de Chimie Médicale*, October.

Arsenic in Commercial Sulphuric Acid.—In the same journal, M. Blondlot states that the sulphuric acid manufactured in the districts of Nancy, in France, has the contamination of a minute quantity of arsenic.

Poisoning with Metallic Arsenic.—Dr. Schutte, of Wolfenbüttel, relates a case of poisoning by metallic arsenic (*Cobalt mineral*), and states, that after having searched toxicological writings, he finds only one instance of the kind on record, in which it was accidentally taken. In the case before us, the poison was intentionally administered by the husband of the deceased. The symptoms produced were those usually produced by arsenious acid, and proved fatal in five days. The metallic arsenic had been given in the form of a coarse powder. In its mineral condition, this substance does not exert a poisonous action, but after exposure to the air, a portion becomes oxidized and converted into arsenious acid, in which condition it was found in the stomach of the deceased. In the intestines, it was found in its metallic condition, leaving the inference, that oxidation had taken place from the secretions or other contents of the stomach. Dr Schütte, however, has ascertained that commercial cobalt mineral contains from 4 to 11 per cent. of arsenious acid.—*Casper's Vierteljahrschrift*, October.

Death from Chloroform.—At the University College Hospital, the administration of chloroform was deemed desirable, in order to facilitate the introduction of the catheter, in the case of a man aged 29 years, suffering from retention of

urine. On its full action having been induced, the patient's breathing became stertorous, and the face suffused. Notwithstanding the use of every means by Mr. Erichsen, the patient died in a few seconds. On examination, it was found that the heart was large and flabby, weighing twelve ounces. The muscular fibre had undergone fatty degeneration, the transverse striæ having been converted into small fatty particles.—*Medical Times and Gazette*, Oct. 14.

Poisoning with Red Precipitate.—Dr. FREDERICK JAMES BROWN, of Chatham, relates the following case. M. A. Kain, aged 16 years and 9 months, swallowed thirty grains of red precipitate mixed with jam, in mistake for medicine, at 11 A.M. August 31st. The swallowing of the powder was followed by a burning sensation (in the mouth and throat?); vomiting occurred in fifteen minutes, when the powder was mostly ejected. Vomiting took place five times before a quarter to 2 P.M., when she was first seen by Dr. Brown. No red precipitate was seen in the matters ejected from the stomach after the second vomiting. Pain was first felt in the stomach, two hours and a quarter after the poison had been swallowed. The treatment consisted in the administration of milk and eggs, and half-grain doses of opium. The mouth and throat were inflamed on 1st Sept. Salivation existed on the 4th Sept. From this date she rapidly recovered.—*Association Journal*, Oct. 27.

Toxicological Action of the Vegetable Acids.—Dr. BENCE JONES refers to the action of oxalic acid as being the type of the action of the vegetable acids on the system. The mineral acids, Dr. Jones remarks, altogether differ from this type, their poisonous action differing in kind from the sedative action of oxalic acid, although agreeing in the corrosive action which they possess. The chemical composition of the vegetable acids, as shown in a table given by Dr. Jones, shows so close a chemical relation, that, on these grounds alone, the possibility of the pre-existence of oxalic acid in these acids has been entertained. Thus, for example, tartaric, citric, and valerianic acids have been held to be coupled or conjugate acids, in which oxalic acid is ready formed.

Dr. Jones cites the recorded action of oxalic and other vegetable acids in repeated minute doses, as well as in larger quantities. Oxalic acid has been found to possess narcotic properties, and when long continued in small diluted doses, closely resembles the effects produced by acetic acid. The actions of acetic acid, tartaric acid, and citric acid, in various doses, are stated by Dr. Jones, and the resemblance between these and the operation of oxalic acid pointed out, and illustrated by observations of the striking resemblance between some other compounds of carbon having narcotic properties—e.g., carbonic acid, $C\,O_2$; oxalic acid, C_2O_3; carbonic oxide, $C\,O$; light carburetted hydrogen, $C\,H_2$; olefiant gas, C_2H_2; cyanogen, $C_2\,N$; prussic acid, $C_2\,N\,H$. The less oxygen the more poisonous the substance. Hence the activity of the substance would appear to depend on the carbon rather than on the oxygen. It is not improbable that, if pure carbon were not insoluble, it might be found to be an energetic poison.

The lecture from which the preceding facts are borrowed contains much valuable practical information regarding the medicinal action of lemon juice and citric acid. —*Lectures on Materia Medica, by Dr. Bence Jones, in Medical Times and Gazette*, October 21.

M. CHEVALLIER, in the October number of the *Annales d'Hygiène*, has related a case of poisoning by the vapours of carbon, and gives the conclusions of his experiments, confirming the preceding observations of Dr. Jones. M. Chevallier shows that 3 or 4 per cent. of carbonic oxide will suffice to destroy a strong dog, that would not have been killed by less than 30 or 40 per cent. of carbonic acid in the air. Warm-blooded animals may be destroyed by 1 per cent. of carbonic oxide.

Poisoning with Ranunculus Acris.—Some children amusing themselves in a meadow by making coronets of buttercups (*boutons d'or*), one of them was tempted

to eat several of the flowers. A few minutes afterwards, this child was seized with severe colic, and all the symptoms of poisoning, which, however, were removed by medical care.—*Journal de Chimie Médicale*, Octobre.

Slow Poisoning with Copper.—Dr. CORRIGAN states that fatal results may follow slow copper-poisoning, by its effects in undermining the constitution, producing debility, emaciation, and proclivity to the exciting causes of disease. Retraction of the edge of the gums, with purple discoloration, is observed in these cases, and continues for a very long period.

Poisoning with Verdigris.—Dr. REINHARDT, of Ulm, relates the following case, which is interesting from the large quantity taken and the success of treatment. J. K., 26 years of age, was admitted into the Garrison Hospital in consequence of an attempt to poison himself with verdigris, of which he stated that he had taken, on an empty stomach, about one ounce and a half, with some bread and water, at 1 o'clock, P. M. In about half an hour he felt ill, and vomited some of the verdigris. In about three quarters of an hour the vomiting recurred; in the interval, he suffered severe pain over the eyes, and colic-like pain in the stomach, which was somewhat relieved by an oleaginous mixture. On his way to the hospital in the evening, he vomited several times. On admission, he complained of pain in the head, a metallic taste in the mouth, and colicy pains, with feeling of distension of the abdomen; the left hypochondrium was tender to pressure, the pulse was normal, as was also the temperature of the skin. Sugared water and the albumen of four eggs were administered. In about half an hour he vomited some green fluid, with many particles of verdigris in the matters vomited. Sulphuret of calcium was then given. Some abatement of the symptoms took place: these, however, recurred with increased severity about 3 o'clock in the morning, with the addition of ardor urinæ. Leeches, sulphuret of potassium, albumen, &c., were continued. The patient was convalescent by the 8th November. No copper could be discovered in either the saliva, urine, or blood. In the fæces a slight trace was found.—*Henke's Zeitschrift, dritter Vierteljahrschrift*, 1854.

Poisonous Properties of Delphinine.—Van PRAAG, in the *Archiv fur Patholog.*, *&c.*, iv. 3, 385, has given the results of his experiments on fishes, frogs, birds, and mammalia. The effects were almost instantaneous paralysis of the heart. On dissection, congestion of the membranes of the brain, of the heart, the large venous trunks, and of the liver, was observed.—*Quoted in Dr. Littlejohn's Report on Toxicology, in the Association Journal*, Nov. 24.

Poisoning by Oil of Bitter Almonds. Recovery.—Dr. PURSELL, of Kennington, relates the case of a boy, aged four years, who accidentally swallowed about four or five drachms of genuine concentrated oil of bitter almonds. He immediately became insensible; the countenance flushed; eyeballs greatly protruded from the sockets, and having a rolling movement, pupils dilated and insensible to light; pulse slow, full, and strong; breathing stertorous; complete opisthotonos; rigidity of muscles of jaw; frequent convulsive action of muscles of face and neck. Vomiting was produced by sulphate of zinc,—warm water, injected with the stomach pump, removing undigested food strongly flavoured with oil of almonds. Cold affusion, stimulants, external warmth, promoted reaction. Consciousness returned in about four hours.—*Association Journal*, Sept 29, 1854.

Dissection and Chemical Analysis in Elucidation of the Cause of Death, in a case of suspected Poisoning with Cyanide of Potassium.—The body was that of a youth about nineteen years of age, well made. The cornea had undergone scarcely any change. The mouth was firmly closed, and exhibited the dried remains of frothy saliva on the lips, and a brownish coloured fluid flowed from the angles of the mouth. The hair of the scalp was readily extracted. Incipient putrefaction was observed on the abdominal integuments. The posterior aspect of the body presented the usual discoloration from gravitation of blood. On dividing the scalp

a tolerable quantity of thin black blood flowed from the occipital regions. The bones of the skull were thin. The vessels and membranes of the brain were loaded with black blood. The cerebral substance exhibited excess of bloody spots when cut through. The substance of the brain was firm. No odour was perceptible within the cranium. No injury was detected in any of the bones of the skull.

The abdominal cavity contained a quantity of bloody and watery fluid, a portion of which was set aside for analysis. There was increased vascularity in parts of the mucous surface of the small intestines, which presented a dark red colour. The other abdominal viscera were in a healthy condition. The stomach was tied at each end, and removed: a bluish-red discoloration was observed on its upper surface. The remains of food were found in the stomach; the mucous membrane of this organ had an intensely dark reddish-brown colour. The larger veins of the abdominal viscera were somewhat congested with dark blood.

The heart was firm; the large vessels full of blood. The cavities of the heart were empty, with the exception of the right auricle, which was filled with dark blood. The lungs, more particularly the lower lobes, were congested with dark blood. and presented several spots of extravasation.

The upper end of the œsophagus, to the extent of one inch, presented its normal appearances; from this point, however, to its termination, it had a blue colour, not removed by washing in water.

The matters submitted to chemical analysis were,—1. The stomach and its contents; 2. The fluid from the abdominal cavity; 3. Fluid which escaped from the mouth and nostrils; 4. Blood from the cranial cavity; 5 A cup containing matters vomited by deceased; 6. The remains of the food partaken of by deceased. The analysis was performed by Drs. Tschepke and Eichhorn, who detected abundant evidence in 1, 2, and 3, of the presence of prussic acid, supposed to have been derived from ferro-cyanide of potassium, in use in the factory in which deceased had been employed. No prussic acid could be separated from 4, 5, or 6.

It was, for the following reasons, concluded that death in this case had been caused by cyanuret of potassium:—1. It had taken place suddenly; no other poison with which the public is familiar has so sudden an operation as has prussic acid. Strychnine, brucine, atropine, nicotine, are not well known to the public. 2. The unusually slight change that the cornea had undergone. 3. The looseness of the hair of the head. 4. The unusually fluid state of the blood. 5 The congested state of the venous system 6. The inflamed state of the stomach and intestines. 7. The extravasated spots in the lungs.

The rapidity of decomposition of the body, which has been observed in poisoning by prussic acid, was not noticeable in this case, probably owing to the state of combination of the acid with an alkaline base.—*Casper's Vierteljahrsch.*, July.

MEDICAL INTELLIGENCE.

The Cause of the late Explosion at Newcastle.

DR. ALFRED TAYLOR was deputed by the Secretary of State to investigate the circumstances of the late catastrophe at Newcastle. The inquiries were more especially directed to ascertain the truth of a rumour, that large quantities of gunpowder, stored in one of the warehouses or vaults, was the cause of the explosion. We have condensed the report considerably, but have not omitted any important point.

After giving the analysis of a powder found on the premises, which was supposed to be gunpowder, but which was merely a mixture of sulphur, sulphuret of sodium, sulphate of soda, and a little oxide of iron, Dr. Taylor proceeded to the report on the cause of the explosion.

"*Arguments for Gunpowder.*

"The effects produced—suddenness, violence, and extent of the explosion—apparently no other cause in the first instance to account for it.

"*Against Gunpowder.*

"1. No direct evidence of its presence in the warehouse.

"2. No *body of flame* seen at the *instant* of *explosion*:—only before and afterwards.

"3. No traces of *burning by gunpowder* on the bodies or dresses of the dead.

"4. There were *no traces of gunpowder*, consumed or unconsumed, in any part of the premises.

"5. The explosion, from whatever cause, must have taken place in the *vault*—this being the crater, focus, or centre of explosion.

"The water of the crater (the vault) has been analyzed, and contains no salts of potash (the indication of burnt gunpowder). It is surrounded by large blocks of sulphur, some of which has undergone fusion. This sulphur in the vault must have been in a melted state before the gunpowder was fired, because sulphur melts easily at 230 deg. Fahrenheit, and I have determined by experiment that gunpowder is not exploded under a temperature of 525 deg. Fahrenheit.

"The facts being so, the eight tons of gunpowder, or whatever the quantity, must have been discharged in the midst of the melted sulphur, so that the cloud of *charcoal* and *sulphuret of potassium* would have to traverse the melted sulphur; yet analysis shows that there is not an atom of charcoal in the fused sulphur of the crater, nor any sulphuret of potassium or sulphate of potash.

"Every atom of the 2240 lbs. of charcoal, as well as of the 9000 lbs of sulphate of potash, has been proved to be absent from about the centre or focus of explosion.

"The mass of *fused sulphur* from the crater was analyzed. Its chemical composition bears out exactly the statement of the witnesses, that there was nothing in the vault but *sulphur* and *nitrate of soda*.

"Within six yards of the crater, in the precise direction which the explosion took across the Tyne—i.e., north—were lying a number of bags of nitrate of soda, which had evidently been blown over by the explosion As, from their position, they must have been in the very course of the explosion, it is clear that the blast of eight tons of gunpowder could never have passed over them without scorching, singing, and blackening by charcoal the hempen fibre, yet they presented no traces of such action

"6 The sulphuret of potassium produced in the discharge of gunpowder has a tendency to discolour or turn brown white-lead paint—i.e., if within reach of the cloud of sulphuret formed from the discharge of powder. A marine-store shop in Hillgate has had its front blown in, but there is no general tarnish or discoloration of the painted joists.

"For these reasons I do not see that the theory that this explosion took place as the result of gunpowder has any scientific support whatever.

"*For Aqueous Vapour or High-pressure Steam.*

"In favour of this view we may notice the terrific effects—the rending and destruction of all surrounding buildings and objects.

"*Against it.*

"1. A large quantity of water, say one or two tons, must have been suddenly converted to steam in order to produce the effects observed.

"2. How could this have entered the vault in such a volume at once? How could it have accumulated to explode all at once in a quantity sufficient to account for the tremendous explosion? There was no boiler or vessel in the vault to hold these tons of water until it had reached that amount of pressure which would suffice to rend the walls asunder.

"3. At the moment of the explosion, clouds of steam would have been visible,

and some of the bodies at least, near the seat of explosion, would have presented marks of scalding.

"The facts above-mentioned show that steam was not the agent in this case.

"*For Gaseous Explosion.*

"1. The adequate production of a large quantity of gas (nitrogen and sulphurous acid) in a strong and comparatively closed vault, by the combustion of the materials contained in this vault—namely, nitrate of soda and sulphur.

"2. The known law in physics, that when any gases are produced by chemical decomposition in large quantity, in a comparatively confined space, and under a high temperature, the vessel or space must, sooner or later, burst or be rent asunder, simply by the mechanical power of the gas.

"3. The open doorway forms a small area compared with the large area of the vault, and the top of the doorway was five feet from the roof, forming an enclosed space above.

"4. The sulphur will burn at the expense of the oxygen of the nitre; and in proportion to the amount of oxygen or nitre commanded by the sulphur, so will be the intensity of the heat. Eighty-six pounds of nitrate of soda would be equivalent to 2400 cubic feet of air in burning with sulphur.

"5. Assuming that about two tons of this material are in a state of active combustion, so much of the two gases might be suddenly extricated that the chamber, and all above and around it, would be blown to pieces before the gases could find a sufficient escape by the doors. If, by any accident during the conflagration, the space of the doorway became obstructed, partially or wholly, this would at once convert the chamber into a vast shell, which would burst and carry all before it at its weakest point.

"6. Confirmatory of this view, I found that the *powder* scraped from a large mass of stone which had been blown across the Tyne consisted of sulphur, sulphuret of sodium, and sulphate of soda.

"An iron kerb-post, weighing three cwt or more, blown into a stonemason's yard at Gateshead, had on it a powder which I examined, and found to consist of similar constituents—sulphur and sulphate of soda.

"On the gunpowder theory, this appears inexplicable, unless we suppose that all traces of the burnt powder had been carefully washed off. On the theory above given, these deposits on the stone and iron are accounted for.

"7. This kind of explosion is unattended with flame or steam. The gases do not burn (nitrogen and sulphurous acid). They would not (like exploded gunpowder) blacken, scorch, or singe bags of nitrate of soda over which they passed.

"There would be no such volume of flame in the explosion of these gases as in the burning of gunpowder. All would be, at the instant, darkness; and there would be the choking, stifling sensation of sulphurous acid.

"8. How did the sulphur become ignited? It should be known most distinctly and clearly, that sulphur will melt and take fire without the immediate contact of flame, or of any incandescent or ignited body. Hot air alone will cause it to melt in flame and burn. It melts at 230 deg.—i.e., only 18 deg. of Fahrenheit above the boiling point of water; and I have found it to ignite between 347 deg. and 400 deg —certainly under 450 deg. It will ignite at a temperature at which gunpowder *will not* explode.

"My belief is that a temperature of 600 deg. soon spread over the whole of the building, including the basement chambers. There was a ton and a half of coal oil or naphtha in the flat above the vault, which, by burning, must have diffused a large amount of heat through the whole building, quite enough to ignite sulphur. From the worsted factory wall, if the bricks became heated, the heat would be conveyed to the air in a narrow passage described to run out and open by a free space near the mouth of the vault. This air might soon become hot enough to ignite sulphur without the necessity of any burning ember or flame reaching it, or without the wooden roof of the vault being burnt through.

"9. Many of the articles in the basement escaped burning, and much of the lead escaped melting. The burning of bodies depends on the presence of oxygen in the air. Sulphur, when once ignited, soon removes the whole of this oxygen, so that no combustion can go on; and it would itself soon cease to burn, unless it were in contact with a substance like nitrate of soda, which can supply to it an abundance of oxygen.

"I have found that sulphurous acid, even when it forms only 33 per cent. of air, extinguishes a lighted candle, a lighted stick, and all bodies that are kindled or in the act of burning. In the concentrated state, nothing can keep ignited or lighted within this vapour for one single instant. Thus the thick wooden roof of the vault might ignite from above, but it could not be ignited from below, because the upper part of the vault would be quite filled with the vapour of sulphurous acid, and there would be no *air* in the chamber to sustain the combustion of a single spark in the timber. This would explain the non-ignition and absence of burning in the bags of nitrate of soda outside the chamber.

"It is my belief, judging from all the facts as they at present appear in this remarkable case, that the burning of the sulphur and nitre in the chamber might attain the vigour necessary to lead to explosion before all the sulphur outside the chamber was melted—before all the bags outside the chamber were burnt, or the nitrate of soda within them melted—or before all the pigs of lead were melted, and before the roof of the chamber was burnt or had given way. If the flat above fell upon the roof of the vault immediately before the explosion, this would add to the superincumbent weight, increase the explosive force of the gas within, and lead to a greater amount of destruction than would otherwise have occurred.

"This is the only theory upon which, as facts at present stand, I can account for this explosion. Gunpowder does not account for it, unless we throw aside all experience concerning the chemical action of this body. Steam cannot account for it. Lastly, upon the only theory reconcilable with the evidence of what is proved to have been in the vault, and to have been carried by projectiles across the Tyne, and to a great distance in Gateshead, had there been substances in this vault not ignitable, or which could not form gases, there would have been reason for looking to some other force, such as gunpowder, steam, or volcanic agency for a cause.

"Alfred S. Taylor, M.D., F.R.S."

The verdict of the jury was in accordance with this evidence.

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